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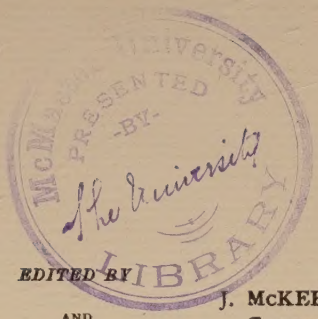
THE Psychological Review



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The Psychological Review.

HERMANN VON HELMHOLTZ AND THE NEW PSYCHOLOGY.

BY PROFESSOR C. STUMPF,

University of Berlin.

Since the death of Darwin, the loss of no one in the scientific world has made such a deep impression as that of Helmholtz. And this is in keeping with that esteem and admiration which in an ever-increasing measure is accorded to his name in the Old and the New World as well, throughout all scientific circles, and in that more practical sphere of life which in the last decade especially has become so largely dependent upon the services of science. From the early beginning of his career, from the time of the anatomical and chemical studies of his youth, all his researches were directed towards high ends, and were crowned with great success. Whenever he smote the rock of nature, there gushed forth the living waters of knowledge. There have always been and are well-rounded, disciplined minds, authors, philosophers, and others, who are able to speak with easy assurance concerning everything in this world, and of much else besides. But they are not for the most part minds which are themselves productive; they are, on the contrary, mere imitators and makers of books. There have always been, and are, moreover, also men of marked scientific note who have proved productive in various widely divergent fields of knowledge; as, for instance, Thomas Young, who in Optics, no less than in the science of Hieroglyphics, rendered considerable service to his time; or, in a similar way, H. Grassmann. But a truly scientific spirit of extraordinary versa-

tility, and yet at the same time evincing complete unity of organization, with all its ideas harmoniously connected one with another, such the world has indeed produced, but only once in the century.

It would hardly be possible for any one man to-day to succeed in understanding all of Helmholtz' investigations equally well. But each will find a peculiar satisfaction in recalling the special impulse which he has experienced in his own department through the inspiration of Helmholtz. And the representatives of Physiological Psychology should feel themselves impelled to such a retrospect, since it is to that science that Helmholtz dedicated himself at the zenith of his power. The two works which have largely rendered his name illustrious, and from which there has proceeded an incalculable stimulus to other men, belong to our especial province. Without in the least depreciating the thoroughly original and fundamental contributions of E. H. Weber, Fechner, and Lotze, nevertheless it must be acknowledged that these two works, both on account of the scientific consequences which have followed them, and of the general and wide-spread knowledge of their contents throughout the scientific world, have more than all others served to bridge the gulf between Physiology and Psychology — a bridge across which thousands of other men now constantly come and go.

Like Dubois-Reymond, Brücke, Ludwig, Henle, Virchow, Helmholtz also came from the school of Johannes Müller. The latter who, as early as 1822, when still a youth of twenty-one, had defended the thesis 'Nemo psychologus nisi physiologus,' had early broken loose from the chains of the Nature-philosophy of the school of Schelling, although without repudiating altogether the spirit of philosophy. He allied himself with Kant, Spinoza, Herbart. A certain taste for philosophy, at least a friendly disposition towards philosophy, was transmitted to his pupils. But in addition to such an affinity we must also note the mode of exact observation of physical phenomena and the experimental method, which were likewise introduced through Müller into German physiology. As a consequence of this method of re-

search, the scholars rejected the theory of 'Vital Force,' to which the master still adhered; and in this highly important reform of the fundamental conceptions of organic life, fraught with such weighty consequences, lay the principal difference between the new and the old epochs. Helmholtz' first work of the pioneer order, upon the 'Conservation of Energy,' represents this new spirit of inquiry. In the incomparably interesting and stimulating discourse delivered at the celebration of his seventieth birthday, he himself drew attention to the fact that his sole object in this early work had been the critical examination and classification of phenomena in the interests of physiology. On the other hand, the pupils of Müller maintained what was to them the fundamental law in the theory of sense-perception,—the doctrine of the specific energies of the nerves,—although their formulation of it did not include the Kantian elements which were attached to the law as expounded by J. Müller. Helmholtz not only accepted the doctrine in general, but, as is well known, applied it in detail in the spheres of Optics and Acoustics, and maintained the diversity of the specific energies corresponding to differences of quality within one and the same sense. A third generation is now empanelled upon this very hook. Whatever we may wish to substitute for the doctrine, we still find ourselves, in my opinion, involved in great uncertainties and obscurities. But that is beside the point here.

Intimately associated with the above-mentioned theory, as far as the facts go, although not necessarily connected with it, there arose with Müller, and also in his school, the clear and emphatic consciousness of the incompatibility of perception, and of psychological events in general, with the processes of the outer world. Müller had expressed himself, it is true, only in a guarded manner concerning the nature of the soul and its relation to the body; moreover, his ideas upon this subject can be made to agree with those of his followers as little as their's in turn with each other. But in two respects at least they were wholly at one: that psychical activities occur only in strict correlation with the physical, and yet that they are throughout

peculiar both in their nature, and in the minor laws of connection to which they are subject. Such formulas as the following were wholly foreign to their thinking: that consciousness is mere appearance, or really nothing (as the modern followers of so-called 'Parallelism' often express themselves in curious inconsistency. Indeed, the doctrine of the complete reality and unique peculiarity of the psychical phenomena may be regarded as a characteristic feature of that epoch.

While J. Müller, after the appearance of his *Handbuch der Physiologie*, turned his attention more to the development of Comparative Anatomy, Helmholtz' investigations, in accordance with his natural gifts, took quite another direction. He was naturally qualified to be a mathematical physicist of the first rank. While his class was reading Cicero in the Gymnasium, he had been computing the path of light beams through the telescope, and establishing certain propositions which were later used by him in his invention of the Ophthalmoscope. He then became a physician for wholly practical reasons, although by no means contrary to his own tastes; yet after the completion of his great works on psychophysics he again turned his attention to mathematical physics. His most essential reforms in the theories of hearing and sight are due to his command of mathematical principles, in connection, to be sure, with an unusual inclination and aptitude for psychological analysis and with an extraordinary inventive talent for the construction of apparatus. In the latter respect, he himself makes the interesting observation, that his youthful predisposition for geometrical methods of treatment was developed in the course of his many experiments into a kind of mechanical intuition; he could feel, as it were, how the angles and lines distribute themselves in a mechanical contrivance, a peculiarity which is also often found in experienced mechanics and machinists.

The '*Physiological Optics*' appeared in parts during the decade, 1856–66. The studies for this undertaking naturally extended to a much earlier date. The invention of the Ophthalmoscope in 1851 was already an incidental fruit of his exhaustive studies in dioptrics. Then followed the sev-

eral publications, upon the Theory of Color, upon Accommodation, upon the Telestereoscope. The great activity in optical investigations, especially in the fifties and sixties (consider, for instance, such names at Brücke, Dove, Listing, Volkmann, Chevreul, Plateau, Fechner, Brewster, Wheatstone, Maxwell), makes the high standard which Helmholtz' work attained more easily comprehended, and increases our astonishment at the intellectual force which was able to gather under new and quickening points of view this wealth of manifoldly divergent results, both native and foreign, and work them into a consistent whole. We must not overlook the fact also that Helmholtz took scarcely any results from others without independent verification. Then the subject of Dioptrics, which had been to a certain extent already written out, received thorough revision, both theoretical and experimental, at his hands. The methods were perfected in both their applications (Opthalmometer, Microoptometer), all the constants in the investigation of the eye more accurately determined, and the mechanism of accommodation cleared up in its essential features.

In the subsequent portions of the work in which Helmholtz establishes the peculiar distinction between sensation (as of color) and perception (as of space), the following form the principal features: the revival and detailed elaboration of Young's Theory of Color—the first comprehensive discussion of the complex relations of color sensations—and the development of the empirical theory of space. The two theories have passed into nearly all the text-books of Physiology, and have had numberless popularizations; the very best, indeed, of these being by Helmholtz himself, whose *Populär-wissenschaftliche Vorträge* call out our ever-increasing admiration. Throughout the extensive range of the popular science literature of Germany, there are very few counterparts to these lectures. They are unpretentious and yet, while in good taste, sufficiently ornate; they preserve as admirably the golden mean between excessive diffuseness, on the one hand, and too rigid concentration on the other; they are equally free from anecdotes and rhetorical extravagances, and especially free from the common method of first

caricaturing old or opposed views in order to set them aside with superior wisdom as childish and absurd.

Since then, as is well known, the theory of color has been zealously attacked from many quarters, and other theories, especially that of Hering, have been opposed to it. The decision in this controversy is still in abeyance. It would not be becoming for me to enter the lists for either side in this connection. To psychologists, however, the question should present itself more strongly than heretofore, whether the so-called composite colors, in distinction from the primary colors, really result from a number of simultaneous sensations, after the analogy of a chord of tones, or whether, on the other hand, these combined color appearances which originate in one and the same place on the retina, constitute an absolutely simple sensation. If I understand them aright, both Helmholtz and Hering uphold the former view, while the majority of psychologists support the latter, and there has been no adequate discussion of the matter. For each investigator seems to consider his own view self-evident.

An especially attractive part of Helmholtz' theory of color, from a psychological point of view, is his explanation of simultaneous contrast. He here introduces a principle which he also turned to good account on other occasions: that, by virtue of our past experiences, we often come to judge and designate objects of sense as quite different from what they really appear to us at the moment to be.

Notwithstanding the masterly elucidation of this principle, the conviction has now become quite general, principally through the exhaustive researches of Hering, that Helmholtz, through a combination of circumstances, was led to resort to an artificial explanation instead of postulating a simple reciprocal action of contiguous nerve-elements upon one another. This leads us to see that it is the sensation itself which is altered, not the judgment alone. Nevertheless, this principle of explanation is so far forth of value, and it remains an important fact that deceptions respecting the nature of sensation which can be very well distinguished from real illusions, are often brought about through experience.

It was also in this book that Helmholtz first made use of 'unconscious inference' in his explanations, although the idea and the theory itself were first definitely stated in connection with his theory of space. The process through which such false judgments, as that of color-contrast, are produced, seemed to him to be analogous to the process of true inference. The misuse which many naturalists and philosophers, who had felt the influence of Schopenhauer, made of this theory, to bolster up various shallow views, gave Helmholtz an occasion later to revise his view and to substitute for unconscious inference a process of association, which certainly is truer to the psychological facts. Moreover, we must take into consideration the fact that Helmholtz, in the *Physiologische Optik*, expressed himself guardedly and with some reservation concerning unconscious inference. He says: "Although, indeed, the similarity of these psychical phenomena with those of conscious inference has been disputed, and perhaps will continue to be disputed, still the similarity of the results is not at all in doubt."

In the theory of space perception Helmholtz departs wholly, as is well known, from Joh. Müller, and erects an imposing structure on quite a different plan. Müller had made the perception of the third dimension a matter of empirical judgment, but he had not determined the necessary moments of the process precisely. It was probably the investigations of Wheatstone in single vision with non-identical points and the observations of single vision in certain squint-eyed persons, together with Lotze's ingenious disquisition upon the space-problem, which gave rise to the first tendency in this direction. Helmholtz opposes the empirical to the nativist theory. Even those who are of the opinion that no one can be wholly just to the established psychological facts without certain concessions to the theory of nativism will, nevertheless, still most readily acknowledge the credit which Helmholtz has won for himself in his treatment of this cardinal question. The antithesis between nativism and empiricism has still more extended bearings; it appears in nearly every psychological question; it characterizes entire schools which have been opposed to one

another, especially in English psychology, for a long time. The exhaustive investigation of the materials for the theory of space from an empirical standpoint has greatly lightened the critical examination of the same for all subsequent thinkers. Always in the habit of searching for certain and clear criteria, Helmholtz sought some characteristic mark to distinguish between that which is a real sensation and that which arises from some supplementary experience. That is, he recognizes as sensation only that part of our intuition which can not be accounted for through assignable experience processes, which might have produced a contrary result.

Another principle of great value, also, to acoustics, concerns the discrimination of simultaneous sensations. We have been accustomed to regard a total of sensations constantly presented together as a common sign for a single object, and the discrimination of these sensations is thereby rendered more difficult. This principle he applied especially to the explanation of experiences of single-vision. The *Physiologische Optik*, as is well known, contains also his philosophical theory of the relation of our consciousness to the outer world, to which Helmholtz in his *Thatsachen der Wahrnehmung* latterly returned, the theory that the *a priori* self-evident law of causation necessitates the acceptation by us of an outer world; that, however, our knowledge of this outer world must remain essentially a symbolical one, and that our sensations are merely signs of what is real. On the occasion of his 'Jubilee,' Helmholtz spoke in quite an elegiac strain of the reception which these views had received at the hands of philosophers. Without inquiring to what extent the blame may be due to certain weaknesses of his representation, or to what extent, also, to the high plane of discussion and the subtilty of his general concepts, or on the other hand to the dearth of clear thinking on the part of some at least of the philosophers who have not passed through the exacting discipline of physics, we are, however, certain that all his philosophical colleagues felt extremely thankful to him for his gifts in this direction also. In my own personal opinion, at least, the theory of the

symbolical knowledge of the outer world really hits upon the right conception. But be that as it may, at any rate the introduction of such considerations into his work, and the accompanying evidence of the deepest interest in philosophical problems, has been largely instrumental in keeping alive and stimulating such interest among students of nature in all lands.

The number of new investigations which were published after the appearance of the *Physiologische Optik*, and called forth by that work, made a new edition of the book desirable long ago. The author had, however, in the meantime turned to other spheres of investigation, as that of electro-dynamics, and it was not astonishing that he decided only lately upon it, when it was no longer possible for him to give due attention to the large amount of material at hand. The successive parts of this work followed each other at ever lengthening intervals of time. The serious misfortune which befel the aged investigator last autumn upon his homeward journey from America urged him moreover to a careful husbanding of his strength, and so made it possible that he should allow the last portion of the work, which includes the discussion of contrast-phenomena, to appear with no reference to Hering's labors in this sphere. This will always be most deeply regretted. The half of the work which still remains unpublished will have to be simply reprinted, only perhaps with a revision of the bibliographical references.

The second psycho-physical work, *Lehre von den Tonempfindungen*, was published in 1863, during one of the intervals in the publication of the several parts of his first work. Studies extending through many years had preceded also in this field, some of which were already published, *Ueber Klangfarbe der Vocale*, *Ueber Combinationstöne*, etc., also the popular lecture upon '*Physiological Causes of Musical Harmony*,' 1857, in which the outlines of the whole work were sketched. One can hardly picture the immense intellectual labor which the carrying out of two such literary projects simultaneously must have involved. However, the problems in the theory of sensation were reciprocally illumi-

nated, and it was precisely the similarity of the phenomena in the two spheres which led Helmholtz to the common view-point already mentioned above. It was during the attractive period of the days of Bonn and Heidelberg that, as he himself afterwards related, while leisurely tramping over the wooded hills, these ideas flowed in upon his mind.

Indeed this second work seems, externally in the form of its presentation, to betray the influence of such a happy mood and friendly environment. It not only treats of the fundamental principles of art, but is itself truly a work of art, and that, too, without any flowers of rhetoric. It is confined to the simple unfolding of the absolutely essential features of the subject. His mightiest weapon, the mathematical calculations and the deductions from previously published computations, was relegated to the Appendix. In the text itself only the manifest principles of the theory are presented to the reader; he is led step by step from the simplest truths of physical acoustics to their physiological conditions, until finally, deep in the center of the system, he is brought to the consideration of the musical scales and harmony, and over the threshold into the æsthetics of music. Here the author deliberately stops.

In Acoustics, too, the achievements of Helmholtz were not creations out of nothing. The illustrious labors of others preceded them—especially Fourier's demonstration of the possibility of resolving any periodic vibration into a number of simple harmonic vibrations; Ohm's definition of simple tones by means of harmonic vibrations, and his correspondence with Seebeck (in which Ohm approached very near to the correct explanation of timbre); Seebeck's theory of sympathetic vibrations; Wilhelm Weber's investigations concerning reed-pipes; Joh. Müller's experimental studies upon vibrating membranes and the human voice; Chladni's caretaking observations, and finally on the side of the theory of music, Rameau's application of the overtone to the theory of harmony. That the application of the theory of beats in the preceding century for a similar purpose had already been attempted, was probably not known

to Helmholtz. But he was familiar with the the hypothesis of an isolation of tones in the cochlea held by many physiologists of the preceding decade, and Harless in Wagner's Dictionary had in fact treated the organ of Corti accordingly. There is, however, a vast difference between the expression of a thought in general terms and the presenting of it in its concrete setting or in its connection with a mass of facts, and it is a long way from eminent labors along single lines and from a one-sided standpoint, to the comprehensive, all around investigation of this entire field, which inspected all sides of the problem impartially, and went even into the depths of the history of music. The work has passed through four editions. Owing to the comparatively small number of subsequent works, as well as to the almost universal acquiescence in his opinions, it was possible to satisfy the demand for new editions without too great difficulty. The most essential changes concerned the significance of the organ of Corti (from the first to the second edition) and the theory of the psychological conditions of tone-analysis (from the third to the fourth edition). It seems almost a pity to have to acknowledge that even this magnificently constructed work will not in all its main features defy the ravages of time. This is, however, my firm conviction. The explanation of timbre, that old riddle, will, however, remain a permanent acquisition. But whether it was correct to deduce timbre and consonance from one and the same principle, that of the overtones, and to define dissonance by means of beats, that is the chief question.

What human achievement, however, can defy time and know no change? Considerations of a critical nature will not disturb the satisfaction with which we feel that our century has beheld an immense advance of science, and that we ourselves were contemporaries of the man who accomplished it. Only in the last few months has it been the privilege of the writer of these lines to know him. Whoever has been thus fortunate will always hold in lively remembrance the massive head, the large, thoughtful eyes, the repose of manner, the modest and unpretentious bearing.

But we shall always retain as his most valuable legacy the inseparable union of scientific and philosophical research, and the profound conception of the mental life, grounded throughout upon actual facts, and for that very reason the loftier and more truly ideal.*

* Translated from the author's manuscript by Professor John Grier Hibben, Princeton College.

THE THEORY OF EMOTION.

(II.) The Significance of Emotions.

BY PROFESSOR JOHN DEWEY,

University of Chicago.

In a preceding article[1] I endeavored to show that all the so-called expressions of emotion are to be accounted for not by reference to emotion, but by reference to movements having some use, either as direct survivals or as disturbances of teleological coördinations. I tried to show that, upon this basis, the various principles for explaining emotional attitudes may be reduced to certain obvious and typical *differentiæ* within the teleological movements. In the present paper I wish to reconsider the James-Lange, or discharge, theory of the nature of emotion from the standpoint thus gained; for if all emotions (considered as 'emotional seizures,' *Affect*[2] or 'feel,' as I may term it) are constituted by the reflexion of the teleological attitude, the motor and organic discharges, into consciousness, the same principle which explains the attitude must serve to analyze the emotion.

The fact, if it be a fact, that all 'emotional expression' is a phase of movements teleologically determined, and not a result of pre-existent emotion, is itself a strong argument for the discharge theory. I had occasion to point out in my previous article that the facts brought under the head of 'antithesis' and 'analogous stimuli' are absolutely unaccountable upon the central theory, and are matters of course upon the James theory. But this statement may be further generalized. If every emotional attitude is referred to useful acts, and if the emotion is *not* the reflex of such act, where does

[1] Psychological Review, Nov., 1894.

[2] See this Review, Sept., 1894, p. 523.

it come in, and what is its relation to the attitude? The first half of the hypothesis prevents its being the antecedent of the attitude; the latter half of the hypothesis precludes its being the consequent. If it is said that the emotion is a mere side issue of that central excitation (corresponding to the purpose) which issues in the muscular and organic changes, then we are entitled to ask, *a priori*, for some explanation of its unique appearance at this point, some sort of mechanical or teleological *causa essendi;* and, *a posteriori*, to point out that, as matter of fact, every one now supposes that his emotion, say of anger, does have *some* kind of direct relation to his movements—in fact, common usage compels us to speak of them as movements of anger. I think, then, that logic fairly demands either the surrender of the 'central' theory of emotion or else a refutation of the argument of the preceding paper, and a proof that emotional attitudes are to be explained by reference to emotion, and not by reference to acts.

More positively, this reference to serviceable movement in explanation of emotional attitudes, taken in connection with the hypothesis that the emotional 'feel' is always due to the return wave of this attitude, supplies a positive tool for the analysis of emotion in general and of particular emotions in especial. As indicating the need of a further consideration, it may be pointed out that Mr. James himself lays the main emphasis of his theory upon its ability to account for the *origin* of emotions, and as supplying emotion with a 'physical basis,' not upon the psychological analysis which it might yield of the nature of emotional experience. Indeed, James definitely relegates to the background the question of classification,[1] saying that the question of genesis becomes all-important. But every theory of genesis must become a method of analysis and classification. The discharge theory does, indeed, give the *coup de grace* to the fixed pigeon-hole method of classification, but it opens the door for the genetic classification. In other words, it does for the emotions precisely what the theory of evolution does in biology; it destroys the arbitrary and subjective schemes, based on mere

[1] *Psychology*, Vol. II., p. 454 and p. 485.

possession of likenesses and differences, and points to an objective and dynamic classification based on descent from a given functional activity, gradually differentiated according to the demands of the situation. The general conclusion indicated regarding the nature of emotion is that:

Emotion in its entirety is a mode of behavior which is purposive, or has an intellectual content, and which also reflects itself into feeling or Affects, as the subjective valuation of that which is objectively expressed in the idea or purpose.[1]

This formula, however, is no more than a putting together of James' theory with the revision of Darwin's principles attempted in the last number. If an attitude (of emotion) is the recurrence, in modified form, of some teleological movement, and if the specific differentia of emotional consciousness is the resonance of such attitude, then emotional excitation is the felt process of realization of ideas. The chief interest lies in making this formula more specific.

In the first place, this mode of getting at it relieves Mr. James's statement of the admittedly paradoxical air which has surrounded it. I can but think that Mr. James' critics have largely made their own difficulties, even on the basis of his 'slap-dash' statement that "we feel sorry because we cry, angry because we strike, afraid because we tremble." The very statement brings out the idea of *feeling* sorry, not of *being* sorry. On p. 452 (Vol. II) he expressly refers to his task as "subtracting certain *elements of feeling* from an emotional state supposed to exist *in its fulness*" (italics mine). And in his article in this REVIEW (Sept., 1894), he definitely states that he is speaking of an *Affect*, or emotional seizure. By this I understand him to mean that he is not dealing with

[1] In my *Psychology*, *e.g.*, p. 19 and pp. 246–249, it is laid down, quite schematically, that feeling is the internalizing of activity or will. There is nothing novel in the doctrine; in a way it goes back to Plato and Aristotle. But what first fixed my especial attention, I believe, upon James' doctrine of emotion was that it furnishes this old idealistic conception of feeling, hitherto blank and unmediated, with a medium of translation into the terms of concrete phenomena. I mention this bit of personal history simply as an offset to those writers who have found Mr. James' conception so tainted with materialism. On the historical side, it may be worth noting that a crude anticipation of James' theory is found in Hegel's Philosophie des Geistes, § 401.

emotion as a concrete whole of experience, but with an abstraction from the actual emotion of that element which gives it its differentia—its feeling *quale*, its 'feel.' As I understand it, he did not conceive himself as dealing with that state which we term 'being angry,' but rather with the peculiar 'feel' which any one has when he is angry, an element which may be intellectually abstracted, but certainly has no existence by itself, or as full-fledged emotion-experience.

What misled Mr. James' critics, I think, was not so much his language, as it was the absence of all attempts on his part to connect the emotional seizure with the other phases of the concrete emotion-experience. What the whole condition of *being* angry, or hopeful or sorry may be, Mr. James nowhere says, nor does he indicate why or how the 'feel' of anger is related to them. Hence the inference either that he is considering the whole emotion-experience in an inadequate way, or else—as Mr. Irons took it—that he is denying the very existence of emotion, reducing it to mere consciousness of bodily change as such. Certainly, even when we have admitted that the emotional differentia, or 'feel', is the reverberation of organic changes following upon the motor response to stimulus, we have still to *place* this 'feel' with reference to the other phases of the concrete emotion-experience. 'Common sense' and psychological sense revolt at the supposed implication that the emotional 'feel' which constitutes so much of the meaning of our lives is a chance arrival, or a chance super-imposition from certain organic changes which happen to be going on. It is this apparently arbitrary isolation which offends.

If, preparatory to attempting such a placing, we put before us the whole concrete emotional experience, we find, I think, that it has two phases beside that of *Affect*, or seizure. (1) It is a disposition, a mode of conduct, a way of behaving. Indeed, it is this practical aspect of emotion which common speech mainly means to refer to in its emotional terms. When we say that John Smith is very resentful at the treatment he has received, or is hopeful of success in business, or regrets that he accepted a nomination for office, we do not simply, or even chiefly, mean that he has a certain 'feel'

occupying his consciousness. We mean he is in a certain practical attitude, has assumed a readiness to act in certain ways. I should not fear a man who had simply the 'feel' of anger, nor should I sympathize with one having simply the 'feel' of grief.[1] Grief means *unwillingness* to resume the normal occupation, practical discouragement, breaking-up of the normal reactions, etc., etc. Just as anger means a tendency to explode in a sudden attack, not a mere state of feeling. We certainly do not deny nor overlook the 'feel' phase, but in ordinary speech the behavior side of emotion is, I think, always uppermost in consciousness. The connotation of emotion is primarily ethical, only secondarily psychical. Hence our insulted feeling when told (as we hastily read it—our interpretation is 'slap-dash' rather than the sentence itself) that we are not angry until we strike, for the sudden readiness to injure another is precisely what we mean by anger. Let the statement read that we do not have the emotional seizure, the 'feel' of anger, till we strike, or clench our fist, or have our blood boil, &c., and the statement not only loses its insultingly paradoxical quality, but (unless my introspection meets a different scene from that of others) is verified by every passing emotion. (2) But the full emotional experience also always has its 'object' or intellectual content. The emotion is always 'about' or 'toward' something; it is 'at' or 'on account of' something, and this prepositional reference is an integral phase of the single pulse of emotion; for emotion, as well as the idea, comes as a whole carrying its distinctions of value within it. The child who ceases to be angry *at* something—were it only the floor at last—but who keeps up his kicking and screaming, has passed over into sheer spasm. It is then no more an emotion of anger than it is one of æsthetic appreciation. Dis-

[1] I take it that this separation of 'feel' from practical attitude is precisely what makes the difference between an emotional and a sentimental experience. The fact that the 'feel' may be largely, though never wholly, simulated, by arousing certain organic excitations apart from the normal practical readiness to behave in a certain way, has played a sufficiently large part in our 'evangelical' religions. The depth, in a way, and the hollowness, in another way, of the subjectively induced religious sentiments seems to me, in itself, a most admirable illustration of the truth of James' main contention.

gust, terror, gratitude, sulkiness, curiosity—take all the emotions seriatim and see what they would be without the intrinsic reference to idea or object. Even the pathological or objectless emotion is so only to the rational spectator. To the experiencer (if I may venture the term) it subsumes at once its own object as source or aim. This feeling of depression must have its reason; the world is dark and gloomy; no one understands me; I have a dread disease; I have committed the unpardonable sin. This feeling of buoyancy must have its ideal reference; I am a delightful person, or one of the elect or have had a million dollars left me.[1]

It is perhaps at this point that the need of some reconstruction which will enable us to place the phases of an entire emotional experience becomes most urgent. In Mr. James' statement the experience is apparently (apparently, I say; I do not know how much is due to the exigency of discussion which necessitates a seeming isolation) split up into three separate parts: First comes the object or idea which operates only as stimulus; secondly, the mode of behavior taken as discharge of this stimulus; third, the *Affect*, or emotional excitation, as the repercussion of this discharge. No such seriality or separation attaches to the emotion as an experience. Nor does reflective analysis seem to establish this order as the best expression of the emotion as an object of psychological abstraction. We might almost infer from the way Mr. James leaves it that he is here a believer in that atomic or mosaic composition of consciousness which he has so effectively dealt with in the case of intellectual consciousness. However this may be, Mr. James certainly supplies us, in the underlying *motif* of this 'chapter' on emotion, with an adequate instrument of reconstruction. This is the thought that the organic discharge is an *instinctive* reaction, not a response to an idea as such.

Following the lead of this idea, we are easily brought to the conclusion that *the mode of behavior is the primary thing, and that the idea and the emotional excitation are constituted at one and the same time; that, indeed, they represent the tension*

[1] I do not mean, of course, that every 'pathological' emotion creates an intellectual delusion; but it does carry with it a changed intellectual coloring, a different direction of attention.

of stimulus and response within the coördination which makes up the mode of behavior.

It is sheer reflective interpretation to say that the activity in anger is set up by the object, if we by object mean something consciously apprehended as object. This interpretation, if we force it beyond a mere way of speaking into the facts themselves, becomes a case of the psychological fallacy. If my bodily changes of beating heart, trembling and running legs, sinking in stomach, looseness of bowels, etc., follow from and grow out of the conscious recognition, *qua conscious recognition*, of a bear, then I see no way for it but that the bear is already a bear of which we are afraid—our idea must be of the bear as a fearful object. But if (as Mr. James' fundamental idea would imply, however his language may read at times) this reaction is not to the bear as *object*, nor to the *idea* of bear, but simply expresses an instinctive coördination of two organic tendencies, then the case is quite different. It is not the idea of the bear, or the bear as object, but a certain *act of seeing*, which by habit, whether inherited or acquired, sets up other acts. It is the kind of *coördination of acts* which, brought to sensational consciousness, constitutes the bear a fearful or a laughable or an indifferent object. The following sentence, for example, from James (this REVIEW, Vol. I. p. 518) seems to involve a mixture of his own theory with the one which he is engaged in combatting: "Whatever be our reaction on the situation, in the last resort it is an *instinctive reaction* on that one of its elements which strikes us for the time being *as most vitally important*." The conception of an instinctive reaction is the relevant idea; that of reaction upon an element 'which strikes us as important' the incongruous idea. Does it strike us, *prior* to the reaction, as important? Then, most certainly, it already has emotional worth; the situation is already delightful and to be perpetuated, or terrible and to be fled, or whatever. What does recognition of importance mean aside from the ascription of worth, value—that is, aside from the projection of emotional experience?[1] But I do not think

[1] It seems to me that the application of James' theory of emotion to his theory of attention would give some very interesting results. As it now stands, the theory

James' expression in this and other similar passages is to be taken literally. The reaction is not made on the basis of the apprehension of some quality in the object; it is made on the basis of an organized habit, of an organized coördination of activities, one of which instinctively stimulates the other. The outcome of this coördination of activities constitutes, for the first time, the object with such and such an import—terrible, delightful, etc.—or constitutes an emotion referring to such and such an object. For, we must insist once more, the frightful object and the emotion of fear are two names for the same experience.

Here, then, is our point of departure in placing the 'feel,' the 'idea,' and the 'mode of behavior' in relation to one another. The idea or object which precedes and stimulates the bodily discharge is in no sense the idea or object (the intellectual content, the 'at' or 'on account of') of the emotion itself. The particular idea, the specific quality or object to which the seizure attaches, is just as much due to the discharge as is the seizure itself. More accurately and definitely, the idea or the object is an abstraction from the activity just as much as is the 'feel' or seizure. We have certain organic activities initiated, say in the eye, stimulating, through organized paths of association in the brain, certain activities of hands, legs, etc., and (through the coördination of these motor activities with the vegetative functions necessary to maintain them) of lungs, heart, vaso-motor system, digestive organs, etc. The 'bear' is, psychologically, just as much a discrimination of certain values, within this total pulse or coördination of action, as is the feeling of 'fear.' The 'bear' is constituted by the excitations of eye and coördinated touch centres, just as the 'terror' is by the disturbances of muscular and glandular systems. The reality, the coördination of these partial activities, is that whole activity which may be described equally well as 'that terrible bear,' or 'Oh, how frightened I am.' It is precisely

'in attention' of preferential selection on the *basis* of interest seems to contradict the theory of emotional value as the *outcome* of preferential selection (that is, specific reaction). But the contradiction is most flagrant in the case of effort, considered, first, as emotion and then as an operation of will.

and identically the same actual concrete experience; and the 'bear,' considered as one experience, and the 'fright,' considered as another, are distinctions introduced in reflection upon this experience, not separate experience. It is the psychological fallacy again if the differences which result from the reflection are carried over into the experience itself. If the fright comes, then the bear is not the bear of that particular experience, is not the object to which the feeling attaches, *except* as the fright comes. Any other supposition is to confuse the abstract bear of science with the concrete (*just this*) bear of experience.

The point may be further illustrated by the objection which Mr. Irons has brought against the James theory. (Mind, 1894, p. 85). "How can one perceptive process of itself suffuse with emotional warmth the cold intellectuality of another?" Note here the assumption of two distinct 'processes', apparently recognizing themselves as distinct, or anyhow somehow marked out as different in themselves. The continued point of Mr. Irons' objection is that Mr. James makes intellectual and emotional 'states', (values) the knowledge of an object and the emotion referred to it, both due to currents from the periphery, and the same kind of current cannot be supposed to induce such radically different things as an intellectual and an emotional process. The objection entirely overlooks the fact that we have but the one organic pulse, the frightful bear, the frightened man, whose reality is the whole concrete coördination of eye—leg—heart, &c., activity, and that the distinction of cold intellectuality and warm emotionality is simply a *functional* distinction within this one whole of action. We take a certain phase which *serves a certain end*, namely, giving us information, and call that intellectual; we take another phase, having another end or value, that of excitement, and call that emotional. But does any one suppose that, *apart from our interpretation of values*, there is one process in itself intellectual, and another process in itself emotional? I cannot even frame an idea of what is meant. I can see that the eye-touch process gives us information mainly, and so we call that intellectual; and that the heart-bowels process gives us the valuation of this informa-

tion in terms of our own inner welfare,—but aside from this distinction of *values* within a concrete whole, through reflection upon it, I can see nothing.

If, then, I may paraphrase Mr. James' phraseology, the statement would read as follows: Our customary analysis, reading over into the experience itself what we find by interpreting it,[1] says we have an idea of the bear as something to be escaped, and so run away. The hypothesis here propounded is that the factors of a coördination (whether due to inherited instinct or to individually acquired habit) begin to operate and we run away; running away, we get the idea of 'running-away-from-bear', or of 'bear-as-thing-to-be-run-from.' I suppose every one would admit that the complete, mature idea came only in and through the act of running, but might hold that an embryonic suggestion of running came before the running. I cannot disprove this position, but everything seems to point the other way. It is more natural to suppose that as the full idea of running away comes in from the full execution, so the vague suggestion comes through the vague starting-up-of the system, mediated by discharge from the centres.

The idea of running away must certainly involve, as part of its content, an excitation of the 'motor-centres' actually concerned in running; it would seem as if this excitation must involve some, however slight, innervation of the peripheral apparatus involved in the act.[2] What ground is there for supposing that the idea comes to consciousness save through the sensorial return of this peripheral excitation? Is there any conceivable statement, either in terms of introspection or of nervous structure, of an idea of movement coming to consciousness absolutely unmediated peripherally? Sensorial consciousness, mediated by the incoming

[1] This is simply circumlocution for 'common-sense.' Common-sense is practical, and when we are practical it is the value of our experience, what we can get out of it or think we can, that appeals to us. The last thing that concerns us is the actual process of experiencing, *qua* process. It might almost be said that the sole difficulty in psychology, upon the introspective side, is to avoid this substitution of a practical interpretation of an experience for the experience itself.

[2] I do not mean that this innervation comes to consciousness as such; on the contrary.

current, is an undoubted fact; it is *vera causa*. Putting the two hypotheses side by side simply as hypotheses, surely the logical advantage of economy and of appeal to *vera causa* is on the side of the theory which conceives the idea of movement in terms of a return of discharge wave, and against that which would make it a purely central affair.[1]

But this is far from being all. I suppose one is fairly entitled now to start from the assumption of a sensory-continuum, the 'big, buzzing, blooming confusion,' out of which particular sensory quales are differentiated. Discrimination, not integration, is the real problem. In a general way we all admit that it is through attention that the distinctions arise, through selective emphasis. Now we may not only rely upon the growing feeling that attention is somehow bound up with motor adjustment and reaction, but we can point to the specific facts of sensorial discrimination which show, that, as a matter of fact, the range and fineness of discrimination run parallel to the apparatus for motor adjustments. We can also show that, in the only case in which there has, as yet, been a serious attempt to work out the details of discrimination, namely, space distinctions, all hands agree that they come through motor adjustments—the question whether 'muscular' or joint surface sensations are primary, having here no importance. Such being the case, how can the particular stimulus which excites the discharge be defined as *this* or *that* object apart from our reaction to it? I do not care to go into the metaphysics of objective qualities, but dealing simply with the psychological recognition of such qualities, what basis or standard for qualitative definiteness can we have, save the consciousness of differences in our own organic response? The bear may be a

[1] There are further logical grounds for expecting acquiescence from those who accept the general standpoint of Mr. James. To say nothing of the insistence upon consciousness as essentially reactive or motor, 'idea' and emotional seizure hang together. Fear-of-bear, bear-as-fearful-object cannot be separated. Besides, when I introspect for my 'fringe' in the stream of thought I always find its particular sensorial basis in shiftings of directions and quantity of breath, and other slight adjustments, just as certainly as I always can pick out the sensorial basis for my emotional seizures. *A priori*, it is difficult to see what the 'fringe' can be save the feeling of the running accompaniment of aborted acts, having their value now only as signs or cues, but originally complete in themselves.

thousand times an individual entity or distinct object metaphysically, if you please; you may even suppose, if you will, that the particular wave-lengths which deflect from the bear, somehow sort themselves out from the wave-lengths coming from all the rest of the environment, and come to the brain as a distinct bundle or package by themselves—but the recognition of just *this* object out of the multitude of possible objects, of just this bundle of vibrations out of all the other bundles, still remains to be accounted for. The predominating motor response supplies the conditions for its objectification, or selection. There is no competing hypothesis of any other machinery even in the field.

We return, then, confirmed, to our belief that the mode of behavior, or coördination of activities, constitutes the ideal content of emotion just as much as it does the *Affect* or 'feel', and that the distinction of these two is not given in the experience itself, but simply in reflection upon the experience. The mode of action constituted by the organic coordination of certain sensori-motor (or ideo-motor) activities, on one side, and of certain vegetative-motor activities on the other, is the reality, and this reality has a value, which, when interpreted, we call intellectual, and a value which, when interpreted we call Affect, or 'feel'. In the terms of our illustration, the mode of behavior carried with it the concept of the bear as a thing to be acted towards in a certain way, and of the 'feel' of our reaction. It is brown and chained—a 'beautiful' object to be looked at. It is soft and fluffy—an 'æsthetic' object to be felt of. It is tame and clumsy—an 'amusing' object to while away time with. It is hungry and angry—and is a 'ferocious' object to be fled. The consciousness of our mode of behavior as affording data for other possible actions constitutes the bear an objective or ideal content. The consciousness of the mode of behavior as something in itself—the looking, petting, running, etc.—constitutes the emotional seizure. In all concrete experience of emotion these two phases are organically united in a single pulse of consciousness.

It follows from this that all emotion, as excitation, involves inhibition. This is not absolute inhibition; it is not

suppression or displacement. It is incidental to the coördination. The two factors of the coördination, the 'exciting stimulus' and the excited response, have to be adjusted, and the period of adjustment required to affect the coördination, marks the inhibition of each required to effect its reconstruction as an integral part of the whole act. Or, since we have recognized that the exciting stimulus does not exist as fact, or object, until constituted such by the coördination in the final act, let us say that the activities needing adjustment, and so partial inhibition, are the kinæsthetic (sensori-motor or ideo-motor) activities which translate themselves into the 'object', and the vegetative-motor activities which constitute the 'reaction' or 'response' to the 'object'.

But here, again, in order to avoid getting on the wrong track it must be noted that this distinction of 'object' and 'response' is one of interpretation, or value, and not a plain matter of course difference in the experiencing. I have already tried to show that the 'object' itself is an organic excitation on the sensori-motor, or, mediately, ideo-motor side, and that it is not *the* peculiar object *of* the emotion until the mode of behavior sets in, and the diffusive wave repercussates in consciousness. But it is equally necessary to recognize that the very distinction between exciting or stimulating sensori-motor activity and excited or responding vegetative-motor activity is teleological and not merely factual. It is because these two activities have to be coördinated in a single act, to accomplish a single end, and have therefore to be so adjusted as to coöperate with each other, that they present themselves as stimulus and response. When we consider one activity, say the sensori-ideo-motor activity, which constructs or constitutes the bear as an 'object', not in itself, but from the standpoint of the final act into which it merges—the stopping to took at the bear and study it scientifically, or enjoy its clumsy movements—that activity takes the form of stimulus. So the vegetative-motor activity, which is, in itself as direct experience, simply the intrinsic organic continuation of the sensori-motor activity, being interpreted again as a reduced factor of, or contribution to, the final outcome, assumes the form of response.

But, I repeat, this distinction of stimulus and response is one of interpretation, and of interpretation from the standpoint of the value of some act considered as an accomplished end.

The positive truth is that the prior and the succeeding parts of an activity are in operation together; that the prior activity beside passing over into the succeeding also persists by itself, and yet that the necessary act cannot be performed until these two activities reinforce each other, or become contributing factors to a unified deed. The period of maximum emotional seizure corresponds to this period of adjustment. If we look at the deflection or reconstruction which either side undergoes during this adjustment, we shall call it inhibition—it is arrest of discharge which the activity would perform, if existing by itself. If we look at the final outcome, the completed adjustment, we have coördination.

I think it must be obvious that this account in no way runs athwart Mr. James' denial of inhibition as a necessary phase of the *Affect* (Psychology, Vol. II., p. 476, note). He there speaks of inhibition as if it could mean only complete suppression—which is no inhibition at all, psychologically, since with suppression or displacement, all tension vanishes. It is, indeed, a question of primary impulsive tendencies, but of these tendencies as conflicting with one another and therefore mutually checking, at least temporarily, one another. Acts, which in past times, have been *complete* activities, now present themselves as contemporaneous phases of one activity. In so far as they were once each complete in itself, there is struggle of each to absorb or negate the other. This must either occur or else there is a readjustment and a new whole, or coördination, appears, they now being contributory factors. The inhibition once worked out, whether by displacement of one or by reconstruction of both contending factors, the *Affect* dies out.

This sort of inhibition the James theory not only permits, but demands—otherwise the whole relation between the exciting stimulus and the instinctive response, which is the nerve of the theory, disappears. If the exciting stimulus does not persist over into the excited response, we get simply a case of habit. The familiar fact that emotion as excite-

ment disappears with definiteness of habit simply means that in so far as one activity serves *simply* as means, or cue, to another and gives way at once to it, there is no basis for conflict and for inhibition. But if the stimulating and the induced activities need to be coördinated together, if they are both means contributing to one and the same end, then the conditions for mere habit are denied, and some struggle, with incidental inhibitory deflection of the immediate activity, sets in. In psychological terms, this tension is always between the activity which constitutes, when interpreted, the object as an intellectual content, and that which constitutes the response or mode of dealing with it. There is the one phase of organic activity which constitutes the bear as object; there is the other which would attack it, or run away from it, or stand one's ground before it. If these two coördinate *without friction*, or if one immediately displaces the other, there is no emotional seizure. If they coexist, both pulling apart as complete in themselves and pulling together as parts of a new whole, there is great emotional excitement.[1] It is this tension which makes it impossible to describe any emotion whatever without using dual terms—one for the *Affect* itself, the other for the object 'at', 'towards,' or 'on account of,' which it is.

We may now connect this analysis with the result of the consideration of the emotional attitudes. The attitude is precisely that which was a complete activity once, but is no longer so. The activity of seizing prey or attacking an enemy, a movement having its meaning in itself, is now reduced or aborted; it is an attitude simply. As an instinctive reaction it is thoroughly ingrained in the system; it represents the actual coördinations of thousands and thousands of

[1] See James, II., 496–497. But more particularly I should apply to the difference between relatively indifferent and emotionally excited consciousness precisely what James says of the difference between habitual and reasoned thinking. (II., p. 366.) "In the former, an entire system of cells vibrating at any one moment discharges in its totality into another system, the order of the discharges tends to be a constant one in time; whilst in the latter a part of the prior system still keeps vibrating in the midst of the subsequent system, and the order . . . has little tendency to fixedness in time." Add to this that it is necessary to perform a unified act—or reconstitute a single, comprehensive system, and the reality (though strictly incidental character) of inhibition appears.

ancestors; it tends to start into action, therefore, whenever its associated stimulus occurs. But the very fact that it is now reduced to an attitude or tendency, the very fact that it is now *relatively* easy to learn to control the instinctive blind reaction when we are stimulated in a certain way, shows that the primary activity is inhibited; it no longer exists as a whole by itself, but simply as a coördinated phase, or a contributory means, in a larger activity. There is no reason to suppose that the original activity of attack or seizure was emotional, or had any *quale* attached to it such as we now term 'anger'. The animal or our ancestor so far as it was given up without restraint to the full activity undoubtedly had a feeling of activity; but just because the activity was undivided, it was not 'emotion'; it was not 'at', or 'towards' an object held in tension against itself. This division could come in only when there was a need of coördinating the activity which corresponded to the perception and that which corresponded to the fighting, as means to an activity which was neither perceiving nor fighting. The animal growling and lashing its tail as it *waits* to fight may have an emotional consciousness, but even here, there may be, for all we know, simply a unified consciousness, a complete concentration on the act of maintaining that posture, the act of waiting being the *adequate* response to the given stimulus. Certainly,[1] so far as I can trust my own introspection, whenever my anger or any strong emotion has

[1] I have no intention here of constructing, *a priori*, the animal consciousness. I use this merely as hypothetical illustration; *if* unification of activity, then no emotion; *if* emotion, then tension of intellectual recognition on one side and consideration of how to behave towards object recognized on the other. I must add, however, that such interpretations as Darwin's umbrella case (in his *Descent of Man*), as illustrating a rude sense of the supernatural, seem to me most unwarrantably anthropomorphic. Surely, the only straightforward interpretation is, there was interruption of a reaction which had started to discharge, and that such a change in stimulus suddenly set up another discharge totally at cross-purposes with the first, thus disintegrating the animal's coördinations for a moment. Unless the animal recognizes or objectifies the familiar reaction, and recognizes also the unexpected reaction in such a way that there tension arises between the two, there can be no *emotion* in the animal, but simply a shock of interrupted activity—the sort of fit which James speaks of, Vol. II, 420. It may well be that the feeling of the supernatural in man, however, *is* precisely the feeling of such tension—instead of there being an idea of the supernatural, and then an associated feeling of terror towards it.

gained complete possession of me, the peculiar *Affect quale* has disappeared. I remember well a youthful fight, with the emotions of irritation and anger before, and of partial fear and partial pride afterwards, but as to the intervening period of the fight nothing but a strangely vivid perception of the other boy's face as the hypnotizing focus of all my muscular activities. On the other side, my most intense and vengeful feelings of anger are associated with cases where my whole body was so sat on as to prevent the normal reaction. Every one knows how the smart and burn of the feeling of injustice increases with the feeling of impotency; it is, for example, when strikes are beginning to fail that violence from anger or revenge, as distinct from sheer criminality, sets in. It is a common-place that the busy philanthropist has no occasion to feel the extreme emotion of pathos which the spectator or reader of literature feels. Cases might be multiplied ad libitum.

It is then in the reduction of activities once performed for their own sake, to attitudes now useful simply as supplying a contributory, a reinforcing or checking factor, in some more comprehensive activity, that we have all the conditions for high emotional disturbance. The tendency to large diffusive waves of discharge is present, and the inhibition of this outgoing activity through some perception or idea is also present. The need of somehow reaching an adjustment of these two sides is urgent. The attitude stands for a recapitulation of thousands of acts formerly done, ends formerly reached; the perception or idea stands for multitudes of acts which may be done, ends which may be acted upon. But the immediate and present need is to get this attitude of anger which reflects the former act of seizing into some connection with the act of getting-even or of moral control, or whatever the idea may be. The conflict and competition, with incidental inhibition and deflection, is the disturbance of the emotional seizure.

Upon this basis, the apparent strangeness or absurdity in the fact that a mere organic repercussation should have such tremendous values in consciousness disappears. This organic return of the discharge wave stands for the entire effort of

the organism to adjust its formed habits or coördinations of the past to present necessities as made known in perception or idea. The emotion is, *psychologically, the adjustment or tension of habit and ideal*, and the organic changes in the body are the literal working out, in concrete terms, of the struggle of adjustment. We may recall once more the three main phases presented in this adjustment as now giving us the basis of the classification of the emotions. There may be a failure to adjust the vegetative-motor function, the habit, to the sensori-(or ideo-) motor; there may be the effort, or there may be the success. The effort, moreover, also has a double form according as the attempt is in the main so to use the formed reactions as to avoid or exclude the idea or object, setting up another in its place, or to incorporate and assimilate it—*e. g.*, terror and anger, dread and hope, regret and complacency, etc.[1]

I shall not carry out this classification; but further suggest that, in my judgment, we now have the means for discriminating emotion as *Gefühlston*, as emotional disturbance, or *Affect* (with which we have been dealing so far) and as interest.

Interest is the feeling which arises with the completed coördination. Let the tension solve itself by successive displacements in time, *i. e.*, means assuming a purely serial form in which one stimulates the next, and we get the indifference of routine. But let the various means succeed in organizing themselves into a simultaneous comprehensive whole of action, and we have interest. All interest, *qua* interest, it would follow from this, is qualitatively alike, being differentiated simply by the idea to which it attaches. And expe-

[1] Because of the tension, however, these cannot be set over against each other absolutely. All terror, till it passes into pathological fright, involves anger, and anger some fear, etc. All moral experience is only too full of the subtle and deceiving ways in which regret (condemnation) and complacency (self-approbation) run into each other. There is the Pharisee who can maintain his sense of his own goodness only by tension with his thought of evil; or who can make his depth of remorse material for self-gratulation. And there is the sentimental selfish character which disguises its own disgrace from itself by emotional recognitions of the beauty of goodness, and of its own misfortunes in not being able, in the past, to satisfy this ideal. I have never known other such touching tributes to goodness as can proceed from the sentimental egoist, when he gets into 'trouble,' as he euphemistically terms it.

rience seems to verify this inference. Interest is undisturbed action, absorbing action, unified action, and all interests, *as interests*, are equally interesting. The collection of postage stamps is as absorbing, if it *is* absorbing or an interest, as the discovery of double-stars; and the figuring of indefinite columns of statistics as the discovery of the nature of sympathy. Nor is this a pathological principle, as it might seem to be were we to instance merely fads or hobbies. The multiplicity of deeds which demand doing in this world is too great to be numbered; that principle which secures that if only full or organic activity go into each end, each act shall equally satisfy in its time and place, is the highest ethical principle; it is the statement of the only religious emotional experience which really seems worth while—the sense of the validity of all necessary doing. I cannot dwell upon this matter of interest, but I suggest the case of purely scientific interest as crucial. On one side, it seems wholly unemotional, so free from all disturbance or excitation may it become; on the other, it represents a culmination of absorption, of concentrated attention. How this apparent paradox is to be dealt with save on the supposition that emotion (as *Affect*) is the feeling of tension in action, while interest is the feeling of a complex of relevant activity unified in a single channel of discharge, I do not see.

As for the *Gefühlston*, I shall only state the conclusion that would seem to follow from a thorough-going application of the principle already laid down. I do not know that this complete application is advisable, much less necessary, but I share somewhat in the feeling of Mr. Baldwin as expressed in the Nov. number (p. 617) of this REVIEW, that there is a presumption that a unitary principle holds all the way through.[1] At all events, those who have followed me so far may like to see how the hypothesis already propounded might conceivably apply to the case of, say, delight in certain tones,

[1] It hardly seems fair, though, to charge Mr. James with inconsistency because he declines to force his theory beyond the limits of the facts upon which he feels himself to have a sure hold. Surely we may admire this reserve, even if we cannot imitate it, instead of virtually accusing him of giving away his whole case by admitting, hypothetically, the existence of facts whose explanation would require an opposite principle.

colors or tastes, while those who do not accept the hypothesis will hardly be shocked at one absurdity the more.

The suggestion, then, is that the *Gefühlston* represents the complete consolidation of a large number of achieved ends into the organic habit or coördination. It is interest read backwards. That represents the complete identification of the habits with a certain end or aim. The tone of sense-feeling represents the reaction, the incorporate identification, of the successful ends into the working habit. It is not, as I have hitherto indicated, habit as habit which becomes feelingless; it is only the habit which serves as mere means, or serial stimulus. That a given coördination should assume into itself the value of all associated coördinations is a fact of every day experience. Our eye-consciousness takes up into itself the value of countless motor and touch experiences; our ear takes up the value of motor and visual experiences, &c. There is no apparent reason why this vicarious assumption should not become so organically registered—*pace* Weissman—as to become hereditary; and become more and more functionally incorporated into structure.

To sum up:—Certain movements, formerly useful in themselves, become reduced to tendencies to action, to attitudes. As such they serve, when instinctively aroused into action, as means for realizing ends. But so far as there is difficulty in adjusting the organic activity represented by the attitude with that which stands for the idea or end, there is temporary struggle and partial inhibition. This is reported as *Affect*, or emotional seizure. Let the coördination be effected in one act, instead of in a successive series of mutually exclusive stimuli, and we have interest. Let such coördinations become thoroughly habitual and hereditary, and we have *Gefühlston*.

THE MUSCULAR SENSE AND ITS LOCATION IN THE BRAIN CORTEX.

BY PROFESSOR M. ALLEN STARR,

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It is not necessary to present to the readers of this journal any lengthy discussion in regard to the muscular sense. Every psychologist admits that there is a sense of movement which enables us to appreciate, (1) the position of a limb in space; (2) the degree and force of muscular action necessary to change the position of that limb; (3) the power needed to oppose varying resistances to the motion of that limb. It has been thought by some that the muscular sense was materially aided by the tactile sense in the process of accurate guiding and adjustment; it has been held by others that the muscular sense was wholly independent of the other senses and the fact here recorded supports this latter view. It has been held by some, that the centres of perception of the muscular sense in the cortex were identical with the centres of movement in the cortex; it has been held by others, that these centres were separate from one another; and the fact here recorded, supports this latter view.

It is well known that disturbance in muscular sense may be produced by diseases in various parts of the nervous system. Thus we have ataxia or incoördination of movement, not depending upon paralysis of the muscles, but entirely due to a lack of appreciation of muscular sense impressions, from (1) diseases of the peripheral nerves; (2) diseases of the posterior columns of the spinal cord, as shown in locomotor ataxia; (3) diseases of the lemniscus or its radiation in the internal capsule in its course toward the cortex around the Rolandic fissure; (4) general diffuse diseases of the cortex of the brain, such as general paresis. It is evident therefore,

that any defect in the tract conveying muscular sense from the muscles to the brain cortex, will produce a disturbance in the power of coördination.

Hitherto, facts have been wanting to determine the actual position of the termination of this tract in the cortex and the exact location of the muscular sense centres. The following observation, therefore, is one of considerable value, inasmuch as it illustrates the possibility of producing an entire loss of muscular sense by a limited destruction of the brain cortex, without producing at the same time, any disturbance in motor power or in tactile sensibility; and determines the localization of the muscular sense centre for the hand in the parietal region.

The case presents a set of facts quite analogous to those obtained in a physiological experiment and is one of considerable interest.

A young man was brought to the Presbyterian Hospital, suffering from intense headache, to the left of, and somewhat behind the vertex, and from epilepsy. He had been a healthy boy until his fifth year, when he had a severe fall on his head, which was followed by unconsciousness for several hours. Since that time he had never completely gained his mental balance. He had seemed fairly bright at his lessons, and willing to study, but was very easily agitated and accustomed to give way to emotional excitement or passion; his memory was good, but his powers of application somewhat deficient. When he was sixteen years old, he had another fall on his head followed by unconsciousness, and from that time his symptoms were all increased. The headache was very intense, quite constant, and subject to sudden periods of increase. When the pain increased exceedingly, the boy would develop a maniacal condition, in which his actions were extravagant, his speech abusive and profane, and in which he resorted to acts of violence toward his family and employers. These attacks occurred every few days, unless reduced in frequency by the use of bromide of potash; but in spite of treatment, would occur every three or four weeks. After the attack was over, he had no recollection of what had occurred during it, and on several occasions, he lost con-

sciousness during the attack and slept heavily after it; he never had any convulsions. This condition had been present for five years, when he came to the hospital. It was thought that the attacks were of the nature of epilepsy, being of the variety known as the epileptic psychical equivalent, in which condition a sudden attack of mental aberration takes the place of a convulsion, and though the patient is apparently conscious, he subsequently has no recollection whatever of his acts during an attack. The fact that these attacks had developed after a fall on the head, and that he suffered constantly from severe pain at the seat of the injury, led to an attempt to relieve the condition by opening the skull. Dr. McCosh, Surgeon to the Presbyterian Hospital, trephined him and found, upon exposing the brain, a small vascular tumor lying beneath the point of injury, directly upon the brain surface. The size of this vascular tumor was about ¾ of an inch in diameter. It was removed without producing any injury to the surface of the brain, so far as could be determined. The brain was explored by thrusting a needle into it in three directions, in view of the possibility of finding a small collection of fluid beneath the surface; but nothing was found. The boy recovered from the operation rapidly, so that within ten days he was quite well; but immediately after the operation it was found that he had lost his muscular sense in the right hand and arm, below the elbow. Attention was called to the fact by the peculiar awkwardness in the movement of hand and arm. Any attempt to grasp a pencil or glass of water or to pick up a pin, resulted in most excessive motions of an irregular type, without the possibility of carrying out the desired movement, even when guided by sight. The attempt to place his finger upon his nose with his eyes closed, failed; the finger being carried beyond the side of the head and above it; in fact all voluntary guidance of the hand was imperfect. At the same time his strength was as good as ever, his grip was greater in the right hand than in the left, so that the defect of movement was in no way due to an actual loss of power. When his eyes were closed he was absolutely unable to tell what position had been given to his fingers or hand

by the examiner; he did not know whether his hand was open or closed; when his hand and fingers were placed in a position and he was requested to put the other hand in the same position, his eyes being shut, he was totally unable to do so; he was unable to estimate with any degree of accuracy, substances different in weight in the right hand, though able to detect the differences readily with the left hand. It was evident that his awkwardness of movement was largely due to the inability to adjust his motions with the necessary degree of power. At the same time his tactile sense and sensation of temperature and pain were perfectly normal. There was no disturbance of any kind in the face or leg. This condition began to pass off about three weeks after the operation, and at the end of three months, he had recovered his muscular sense entirely. It was therefore evident, that this particular effect had been produced by a small localized injury of the cortex of the brain, which had been subsequently repaired by nature. The exact position of the cortex injured was easily determined, and it was found to be about two inches behind the fissure of Rolando and about an inch and a half to the left of the median line, at about the junction of the superior and inferior parietal lobules. This observation would therefore indicate: first, that the muscular sense centres are distinct in their location from tactile or pain or temperature sense centres; and also from the motor centres; secondly, that they are situated just behind the motor area in the parietal region of the brain.

A LOCATION REACTION APPARATUS.

BY PROFESSOR G. W. FITZ,

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The problem which stimulated to the designing of this apparatus was that of testing the power of an individual to quickly and accurately touch an object suddenly disclosed to him in an unexpected position. In order to make the problem as simple as possible the apparatus (see Figure 1) was so devised that the subject is required to make a movement of the finger from the end of the nose to some portion of the arc of a circle of which he is the centre and whose plane is at the level of his elbow. Three positions were selected to give a wide range of movement, namely., the centre immediately in front and a point on each side at a distance of about 14 in. (A, B and C). The object to be touched consists of a white spot ½ in. in diameter, which may be placed at any one of these points without the knowledge of the subject, a screen being in front, arranged to fall at the proper time and instantly disclose the spot. In connection with this, a pendulum chronoscope is used which measures the interval of time between the falling of the screen and the touching of the white spot.

The apparatus for determining the error is constructed to measure the distance of the centre of the finger (Fig. 2, F) from the centre of the white spot (S) on either side, thus showing the error of the movement executed and its direction. It consists of a horizontal strip (St) of blackened brass 7 in. long, bearing in its centre the white spot (S). This is hinged along one side so that the finger pressure makes an electrical contact (E) to determine the end of the time interval and also releases the clamp controlling the error recording apparatus. Below this are two light arms (GG) pivoted

at a common point directly under the white spot, so that their tips project above the first strip about ¾ in. These arms are connected by a spring (Sp) tending to pull them together, but are held apart in the preliminary position by the

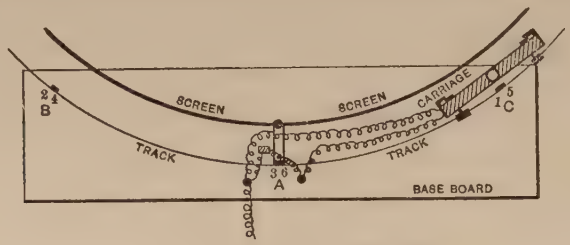

FIG. 1.—LOCATION APPARATUS.

pressure of the clamp projecting downward from strip (St, not shown in diagram) and are released by the touch of the subject, springing instantly to grasp the finger (F) between them. The raising of the finger clamps them anew in this position, and the displacement of the index showing the mid point of the finger can be read on its scale (R–L). This is found to work very quickly and conveniently with practically no observation error. A frame work carries the various parts and a set of wheels enables it to be run into any position desired.

The chronoscope has a balanced pendulum (Fig. 3), 12 inches total length, so weighted (W) that the time of swing is about a second and a half. The pendulum (P) carries a

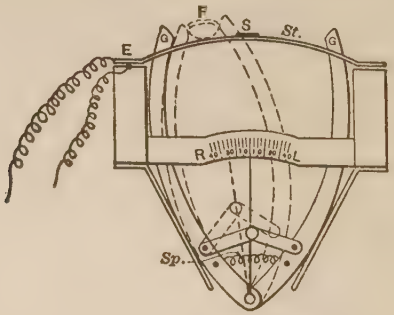

FIG. 2.—ERROR INDEX.

light index (I) that may be clamped instantly in any position on the scale (S), which latter was graduated empirically in hundredths of a second by a falling weight. The pendulum

is held in the preparatory position (Fig. 4) by means of a hook (H) connected with the armature (A) of an electromagnet

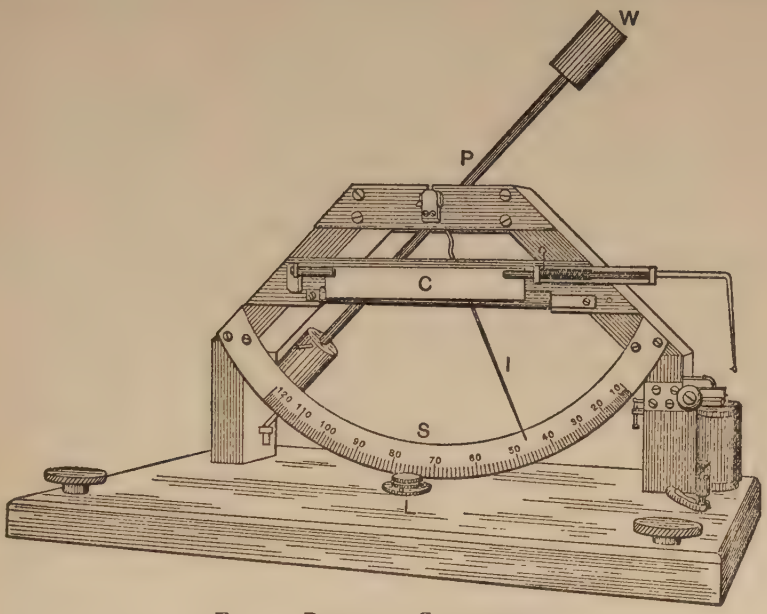

FIG. 3.—PENDULUM CHRONOSCOPE.

(M). The breaking of the circuit by the fall of the screen releases (Fig. 5, R) the pendulum carrying its index; the re-

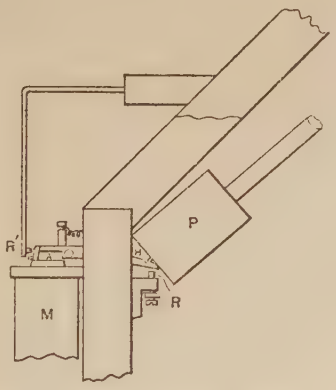

FIG. 4.—PENDULUM AND INDEX CLAMP RELEASE.

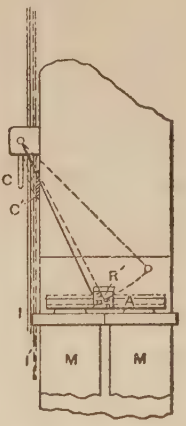

FIG. 5.—CLAMP RELEASE.

making of the circuit, by the touch of the subject's finger, releases (Fig. 5, R) the clamp (C) and catches the index so that

the time may be read upon the scale (Fig. 3, S). There is a level (L) upon the base board to enable one to put it in an exactly horizontal position, and the error of the instrument is thereby reduced to a negligible quantity. The details of the release are shown in Figures 4 and 5.

By means of this apparatus it was hoped to measure some of the elements making up the differences which exist between individuals in their power to do certain things requiring quickness and accuracy, as, for instance, tennis playing and fencing, the essential requirements being the perception and quick interpretation of external conditions, followed instantly by an appropriate motor response. The apparatus gives us somewhat similar conditions to those offered by the games mentioned, but gives them so definitely that it is possible to get a numerical statement of what each individual is able to do in terms of quickness and accuracy. The differences have been found to be remarkably great, and there is an apparent lack of coördination between time and error; that is, those who are quick are not necessarily less accurate than those who are slower.

The accompanying table shows the result of some work with the apparatus, and is given to suggest the wide range of individual ability thus tested.

Time in $\frac{1}{100}$ sec.	Males. No.	Males. % of total.	Males. Av. error.	Females. No.	Females. % of total.	Females. Av. error.
27– 35	11	6.2	11.1	1	1.5	10.
35– 45	48	22.5	10.05	12	18.	9.4
45– 55	54	31.	8.25	18	26.	7.8
55– 65	29	17.	9.	25	35.	7.2
65– 75	18	11.	8.2	11	16.	5.4
75– 85	8	5.	3.1	4	5.	4.4
85– 95	4	2.	4.05	0		
95–105	0	0.	0	1		
105–115	1	.6	7.8			

The tests were made in three positions (A, B, C, in the order 1, 2, 3, 4, 5, 6), as described, every individual making

four attempts in each position, twelve all told. These were recorded separately, hence it is possible to study each effort in relation to the position in which it was made. This was done for both hands to compare the right with the left in regard to quickness, accuracy and direction of error; but it has been thought best not to include a discussion of the results from this standpoint in the present paper.

The table contains a study of the observations made with the right, or preferred, hand by 173 males and 72 females, all those of one individual being treated here as if made in one position. They were obtained from several sources, a large portion of them being derived from the Psychological Laboratory of the Columbian Exposition, Chicago, where the apparatus was in use by Prof. Jastrow, and also from Harvard students and in the Harvard Summer School of Physical Training. Inasmuch as it did not seem possible to make a fair classification of these, they have been arranged in two divisions, male and female, regardless of ages and experiences.

The first column of the table gives the limits of quickness, determining each group of these two classes: the second, in the two divisions of the table, gives the number of individuals whose reactions lie between the limits noted: the third gives the percentage of this number to the total number in the class, while the fourth gives the average error which is a measure of the accuracy of the movements. It will be noticed that the number of individuals of the different groups shows a distinct distribution curve with the apex at about 0.5 Sec. in the males and 0.6 Sec. in the females, suggesting that these are near the means. Of course, this quickness is made up of the reaction—time proper and the time occupied in making the movement from the end of the nose to the plane of the apparatus. It will be noticed also that the average errors for these groups do not vary in ratio to the quickness, but that those who make the movement in .35 Sec. are almost as accurate as the group making the movement in .75 Sec., some being, indeed, more accurate in the former case than in the latter. There is a suggestion of uniformity in the value of the errors, and one cannot help thinking that

the everyday, haphazard activity, demanding as it does a certain degree of accuracy in the execution of movements, determines for each individual his range of error, and that time is the main element of variation.

It will be noted that though the time of the females is longer than that of the males, there is a compensatory increase in accuracy. The relation between time and accuracy has not been determined, so it is not possible to make a statement of the value of accuracy in terms of time, but undoubtedly the individual, who is fairly accurate and very quick, is more accurate when he takes more time, yet it is also true that he is sometimes much more accurate than at others without being necessarily either quicker or slower. These individual variations have still to be studied. The main point to be emphasized now is, that between two persons it is practically possible to bring one element of the test, either time or accuracy, to equality, so that the difference may be expressed numerically in terms of the other. Just what value this series of tests has can not be stated positively, but we believe it has distinct reference to motor ability, and that this will be shown by an increased number of observations upon individuals whose powers are definitely known by comparison with others in the various games.

I wish to acknowledge special indebtedness to Prof. Joseph Jastrow, Mr. G. W. Morehouse, Dr. F. B. Jewett and Mr. A. W. Jeardeau for assistance in getting observations.

DISCUSSION.

MIND AND BODY.

The question of the relation of mind and body is one from which the practiced reader shrinks as from a foreseen and profitless logomachy. It is, so to speak, a game of chess, in which the weary on-looker anticipates every familiar opening and every vain movement to the moment when the infinite baffler of our finite thought mockingly cries 'mate.' The deep-seated intellectual desire for unity and the impossibility of comprehending causal interaction between two disparate substances, drive us to the affirmation of a Spinozistic psycho-physical parallelism. Two courses then lie open to us. Common sense and the dread of paradox lead us to limit this parallelism, and to affirm that it obtains only in the nerve structures of the organic world. That is, that every mental event has its physical counterpart, but that the converse is not true. This view unflinchingly applied, together with the principle of continuous causation, makes the physical world the *prius* and the absolutely real, and reduces thought, feeling and consciousness to phenomenal illusions, *summis fluitantia rebus*. It is perhaps the dominant view as a psychological method at present. But it is not really tenable as an ontological principle by any serious thinker who knows his Berkeley, Kant and Schopenhauer.

The alternative is to make the parallelism absolute and assign a mental and subjective or 'inner' side, not merely to the nerve substances of man and the higher animals, but to every atom of cosmic dust. Mind thus becomes co-extensive with matter, and as the immediately known reduces its physical counterpart to an illusion, an inference, a presentation, an aspect or reflection of itself. But how are we to conceive this mind or minds? Are the barriers and limits imposed by matter as illusory, from the standpoint of the absolutely real, as matter itself? And are all that we count separate minds connected, related and fused in an infinite world-soul that manifests itself in countless finite aspects? Or is our indefectible sense of isolated individuality the mental counterpart of the lines of demarkation we find in the material world; and will eternal form continue to divide the innumerable minds the theory postulates? And if so,

what and where are the essential units of mind and reality? Is the unit the full consciousness of a mature man corresponding to the healthy action of the entire nervous system? Is it one of those split-off consciousnesses of which French pathology has so suspicious a monopoly? Is it a single thought, supposing us to be able to analyze out a single thought? Is it the dim sentience of an amoeba? Is it the postulated inner aspect of a molecule or rather, since molecules are compounds, of an atom? Evidently these questions throw us back into the metaphysics of the Leibnitzian monad, and we are led to speculate whether there may not be two kinds of spiritual unity,—one corresponding to the mental atom, the other to the various aggregations of such units under the control of a superior co-ordinating monad. By this plan, we might return to a world-soul that would yet leave us at least the illusion of a real finite existence. Speculations of this sort, alluring and inevitable as they are to all who dabble in metaphysics, do not touch the realities of our thought and experience very nearly. But however fruitless they may seem, they are at present inextricably involved with the methodology, the aims and the conflicting tendencies of modern psychology. No psychology in recent times is free from this sort of metaphysics, and the writers who protest against it loudest are the most deeply infected. Great interest attaches, therefore, to the review of the entire question just published by the veteran Wundt.[1] If we can not look for final solutions even from him, we may expect light on the darker places of his own voluminous works, suggestive criticisms of present psychological tendencies, and a clear defining of the somewhat obscure issues between him and the young psychologists of the school of Ziehen and Münsterberg.

It is probable that only a few very patient readers have been able to form a clear conception of what the controversy between these two schools, if we may call them so, is about. The new psychologists have all been directly or indirectly trained in the school of Wundt, and the master himself has from time to time modified the formal statement at least of his doctrines in concession to their criticisms. They profess a perfunctory allegiance, which he regards as hollow, to the results of Kantian criticism; and he disavows the traces of supernaturalism or mysticism which they discover in his theories of apperception, attention, and will. Both accept as fundamental the psycho-physical parallelism, the conservation of energy, the indispensability of introspection, the utility of the experimental

[1] *Ueber psychische Causalität und das Princip des psychophysischen Parallelismus.* W. Wundt, *Philos. Stud.*, X., 1–124. 1894.

method. Wundt denies that the psycho-physical parallelism involves the existence of any physical counterpart of the successive creative syntheses by which our thought is qualitatively elaborated out of the elements that analysis detects, and he stigmatizes as materialistic the psychology that fails to recognize this limitation on the study of mind through matter. But Münsterberg, while affirming that the entire content of our consciousness is explicable by the association of sensational elements in obedience to physical laws, expressly excepts from the possibility of such explanation the quality of consciousness that attaches to this content. May it not be that by this inexplicable quality of consciousness Münsterberg is merely generalizing what Wundt means by insisting that every mental synthesis yields a quality that cannot be obtained from the sum of its elements, and which is, therefore, inexplicable by any analysis or synthesis stated in physical symbols? If this is the case, there is, after all, no very serious philosophical difference between the two disputants. Both accept the psycho-physical parallelism, both recognize our inability to bridge the gulf between the two series. But Münsterberg, content with a general recognition of these difficulties, would simplify his psychological analysis by practically ignoring them, as does Spencer, and treating the mere combinations of the elements expressed in physical symbols as an adequate explanation of all higher states. Wundt, on the other hand, wishes us to recognize the irreducible quality of mind at each stage of the synthetic process, by which we rise from the simpler to the more complex mental states.

For the method and the language of psychology, however, the difference is all important. Wundt's psychological descriptions and analyses are couched in a literary language that makes its appeal directly to our conscious experience. The words are chosen for their power to recall vividly to the reader's mind the experience of which he treats. To some extent, of course, all psychologists use language in this way. But the tendency of the 'new,' the 'physiological,' the 'materialistic' psychology is to employ in psychological descriptions and analyses, language which has a merely symbolic and algebraic value, expressions chosen not for their power to reinstate the experience described, but for their convenience for expressing the writer's view of its explanatory analysis. Which dialect will the psychologist of the future use? If he is as clever as Prof. James he will probably employ both languages,—the language of vivid literary description to aid the reader in realizing the states depicted, the language of symbol to lend plausibility and a halo of science to the analysis.

But what is the real service of the symbol? In mathematical physics we substitute abstract symbols for the sensible realities, because the symbol is adequate for our purposes. It enables us to solve problems and to predict results. In other words, it represents the true causal relations so far as they are accessible to human intelligence. Now, the employment of symbols and symbolical, physiological language by the 'young psychologists' is mainly due to their instinctive desire to transfer to psychology the conception of cause which mathematical physics has made the ideal of modern science. Speaking of the schematic diagrams by which he illustrates association, Prof. James says: "It is only as incorporated in the brain that such a scheme can represent anything causal," and he accordingly denies that similarity can be an ultimate law of association on the ground that similar ideas do not co-exist in the mind in the intervals of latency, but are mere dispositions of the brain. A like feeling about psychic causality underlies his suggestions that psychology is awaiting its Galileo or Lavoisier, whose advent may surprise us any day. Our entire conception of the method and prosecution of psychological research will depend on our acceptance or rejection of this assimilation of physical to psychical causality. Owing to the homogeneity of its symbols (which enables us to interpolate imaginary links at pleasure,) physical causation tends to be conceived as a continuous unbroken chain. It is not easy to conceive of psychic causation in this way. Highly complex states succeed each other in the mind with no apprehensible intermediate links, and consciousness as a whole is suspended in sleep and disease. The resort to the infinitesimal of the unconscious savors of Leibnitzian metaphysics. It is easy, then, to see why the young psychologists seek to base their psychologies on the physical conception of cause. But we have still to ask, what is the justification of this procedure, either from the point of view of ultimate metaphysics or of practical psychologizing? If they seriously maintain that all causal efficacy resides in brain states which have no psychic counterpart, then, despite their professed allegiance to the critical philosophy, they are making matter a 'Ding an sich.' If they accept consciousness *in toto*, as a reality for which no physical conditions can account, or if they admit anywhere a mental spontaneity, which can select among the ideas which the associative machinery introduces, they have abandoned the unflinching mechanical explanation of the universe, as completely as if they granted us a soul, possessing the faculty of retaining latent ideas and associating them by similarity. If it is impossible to carry the mechanical explanation through, how

can they define *a priori* the powers and potencies of the irreducible spiritual factor whose presence in the problem is so grudgingly conceded? It will perhaps be said that this is unprofitable metaphysics, and that in practice the 'soul' has shown itself a perfectly barren and useless psychological conception, while brain processes and schematic diagrams have been found to be fruitful working hypotheses. What then is the real outcome, either for knowledge of the mind or for scientific anatomy of the hypothetical brain schemes that adorn the pages of the new psychology? Such are some of the chief problems suggested by Wundt's study, of which, after these introductory reflections, I proceed to give a brief summary.

Viewed in the light of the psychological origin of the conception, a cause is a thing that produces an effect on another thing. When primitive thought has occasion to distinguish cause and condition, it regards the thing as the cause proper and its varying aspects and relations as the conditions of its operation. More exact and abstract thought comes to recognize that there is always some special relation, quality or change of the thing that determines its effect on another thing, and thus arises a tendency to fix the attention on this determining relation or aspect as the cause proper, and to regard things with their complexes of qualities and relations as the conditions. Now in fixing the meaning of a term like cause, we may endeavor to make our definition include its psychological origin and popular acceptation, or we may intentionally modify the conception so as to make it a more convenient instrument of thought. In dealing with the idea of cause we must follow the latter course. We must modify the original conception in accordance with the needs of modern science. To attain command over nature and the power of prediction, science must possess a practicably applicable criterion for distinguishing that condition which is for our purpose the operating cause. From this point of view things are too vague to serve as causes. A thing is a complex of generalities, a seat of countless qualities known or unknown, the center of an infinity of relations, along the line of any one of which its qualities may operate. Yet to disregard things altogether, and to consider qualities and relations only, is to fall back on Hume's conception of causality, and retain no law or order in the world other than subjective rule of habit. This difficulty is in part met in the exact sciences by the modern conception of causality based on the conservation of energy and the equivalence of the forces of nature. The cause equals the effect in units of force, and the chain of causation is a series of mathematical physical equations. What is the significance, and what are the

limitations of this principle? In the present state of science it is a mere postulate. The complication of the problem, if nothing else, prevents verification in the majority of cases. But verification, when possible, is so precise, the evidence accumulates so rapidly, and the satisfaction of the imaginative desire for unity is so complete, that we assume the law to be absolute for the physical world. Does this mean that the physical order constitutes a closed series, into which it is demonstrably impossible to interpolate an alien or spiritual link, such as an impulse from the soul to the brain, however slight? On this point there is an apparent uncertainty in Wundt's utterances. The energy of position to be developed by a stone hurled into the air would remain the same, he says, were the stone arrested by a miracle and held in suspense for a given time. Now, only a small portion of the equations by which the mechanical explanation of the universe is stated are dynamic, we are in every case compelled to rest finally on static equations. And the validity of such equations, while not inconsistent with the conception of the physical world as a closed series, does not preclude the intrusion of an alien form of causality, provided the intruder is not supposed to create, but only to direct or release energy. But surely the only meaning of this is that gravitation, chemical affinity and electricity are still mysteries which we are unable to explain in dynamically continuous terminology. But the modern 'flowing philosophers' will claim that all reality is ultimately expressible in dynamical terms, and that the statical equations are mere temporary expressions of our ignorance. In which case they can only be met by pointing out that on this assumption every problem will be infinitely complicated and commit us to an infinite regress, making the mechanical explanation of the world forever impossible. Wundt virtually admits this. There are two conceivable types of miracles, he says: those that create new energy and those that merely release latent energy. The first are excluded by the law of the conservation of energy. The second become impossible if we postulate a continuous series of dynamic equations between any two statical equations. In any case, the burden of proof rests with the affirmer. Psychic phenomena yield no warrant for assuming miracles of the second kind, and the complexity and purposiveness, which we see in actions, known to be purely reflex and unconscious, are against it. Complete psychophysical parallelism, then, as an empirically given fact and a postulate of method, must be the doctrine of modern psychology. The metaphysical meaning of that parallelism and its application beyond consciousness, belong to metaphysics and epistemology. This

parallelism postulates co-existence in time between the associated members of the physical and mental series. But there are two psychic realities of which it renders no account. These are the combinations of psychic elements with each other and the *Werthunterschiede*. There is nothing in the world of physical forces and processes that corresponds with these.

Here again the young radicals will detect an irrational element in Wundt's philosophy, and it is necessary to define his meaning. Even assuming an established parallelism between the elements, physical combinations cannot explain psychic combinations because the former are quantitative while the latter are qualitative, and the product possesses qualities not found in the elements. This truth, the principle of creative synthesis, as he afterwards calls it, is a sufficient bar to all 'materialistic' psychology. But in the statement we are considering Wundt seems to have affirmed or denied more than is necessary. He denies not only that the physical parallelism can account for qualities, but that there is any physical counterpart. A feeling of inmost union between two tone or color sensations implies, he says, no physical bond beyond contemporaneity. This seems a wanton limitation of the principle of parallelism. The assumption that there is some sort of a physical basis for the qualitative likeness of feelings does not, as Wundt holds, lead to the *reductio ad absurdum* of a Cartesian pineal gland. There is ample room for imaginative conjecture in our ignorance of the structure and functions of the brain. The likeness may be conceived as represented on the physical side, not merely by physical contiguity but through connecting fibres or parallelisms of modes of motion. Such conjectures (which fill so large a place in the psychology of Herbert Spencer) have no anatomical value, but to deny *a priori* their possibility is to fling down the gauntlet against one of the most cherished scientific convictions of the day. Much of the same may be said of the exclusion of *Werthunterschiede* from the parallelism. Nobody claims that their quality is explained by any physical process. But neither is the quality of a taste or color so explicable. The analysis of *Werthunterschiede* into associated elements of pleasurable and painful feeling fills too large a space in contemporary ethics to be thus dismissed with a contemptuous *fin de non recevoir*. The psychic elements of pleasure and pain yielded by this analysis find their parallel in the furtherance or hinderance of the life of the organism. This is not, as Wundt claims, a mere transference of psychic *Werthunterschiede* to the physical side. Furtherance and hinderance,

as Leslie Stephen has shown at length, are defined in this connection by the Darwinian doctrine of survival.

These discussions are followed by an amusing and vivacious critique of the 'materialistic' psychology of Münsterberg, Ziehen and others. By materialistic psychology Wundt understands the psychology that ignores the equal validity of the psychic side of the parallelism, and deduces psychic events from physical. His weightiest criticism, however, is directed not so much against the false metaphysics of the school as against their doctrinaire simplification of the facts of the mental life, their persistent ignoring of the truth that for us the psychic side is in every case the most accessible. The essential vice of their method is that they do not patiently analyze the entire mental life as they find it, but conduct their analysis only with the view of winning hypothetical elements, (Bacon's *advolatio ad maxime generalia*), which are first correlated with elementary physical processes, and then combined in conjectural syntheses to account for everything. All the more complex facts of the mental life are then reconstructed *a priori* by physiological hypotheses about memory and sensory cells, connecting fibres and muscular sensations, without regard to the creative syntheses involved in all mental combinations. Where the quality of the products cannot be ignored it is attributed to the elements, and all possibility of further psychological analysis is precluded. Thus a special spatial and temporal quality is assigned to all sensations *per se*, and our complete intuitions of space and time are explained as summations of these. So Höffding, to account for recognition, assumes a 'quality of familiarity' resting on physiological habit, and all shades of feeling are explained as degrees of painful or pleasurable muscular and vaso-motor reflexes. In all these cases the true psychologist demands in place of this mechanical schematism an analysis of the complex state in mental terms, with distinct recognition and description at each stage of the analysis of the new psychic quality resulting from the combination. Even if we grant a certain symbolic truth to Ziehen's explanation of association by sensory and memory cells and associative fibres, in what way does so obvious a simplification forward our knowledge of the complicated interaction of image and symbol in our higher mental life? Does not the ready-made formula, here as elsewhere, check the patient analysis of detail, which is most fruitful in real knowledge? Wundt does not pause for questions of this kind, but makes merry with the whole theory, gravely suggesting that instead of saying the sensory cell deposits a memory in the memory cell, we should treat the memory

cell as an organism which feeds on the sensory cell, an hypothesis which he elaborates with somewhat ponderous Teutonic wit. Finally he points out that this hypothetical anatomy is of even less value to the physiologist than to the psychologist.

Much of the content of Wundt's final and longest chapter on psychic causality was anticipated in my introduction to this discussion. Specific psychic causality is found in all real processes of the mental life. For the intensive *quale*, the sensation, has real existence for consciousness only in space and time, and space and time, as we have seen, are products of creative psychic syntheses. Wundt's insistence on this point might seem an injustice to Ziehen, who explicitly recognizes that the projection of our sensations into space is "one of those psychological facts that are as yet incomprehensible in the light of physiological psychology, and that perhaps will always remain so." The fact is that in this controversy each party has borrowed all that is available of the dialectical equipment of the adversary, and that while they try to magnify their differences, their real difference is mainly one of taste in the statement of transcendental or irreducible problems. Wundt attaches value to the psychological analysis of our spatial perceptions. Ziehen does not; but it cannot be said that Ziehen attempts to explain space by physiological processes. Similarly of the controversy about memory, reproduction and association. The so-called laws of association are barren formulæ, he tells us. The true explanation of any concrete association is to be sought in the entire content of the consciousness as determined by the totality of its history and original endowment. This of course is an endless series. The physiological psychologists would simplify the problem by substituting the totality of the brain or nervous system, thus shutting us off from all real study of the facts. For psychological analysis can to some extent ascertain the facts in the past history of a given mind that determine a given associative reaction, but psycho-physics is limited to the barren generalization that the total consciousness is a reflection of the total state of the brain. The psychological analysis, however, will always remain on the border line between literature and science, and will not appeal to those who lack Wundt's literary skill or whose minds are dominated by the ideal of causation that prevails in the physical sciences.

In conclusion the author asks if it is possible to formulate definite laws of psychic causality. In the sense in which we speak of Kepler's or Galileo's laws, no. For the most characteristic psychic states are determinations of quality and worth, which do not admit

of quantitative formulation. By way of summing up the whole discussion, however, Wundt sets up two or three principles as specific notes or marks of psychic causality. First, the principle of pure actuality of process. This is in substance an adaptation to modern conceptions of Aristotle's doctrine that thought is pure energy and does not exist as a potentiality. Psychic causes in all cases must be real psychic events, for which it is not possible to substitute 'things' and their potencies, whether in the shape of a substantial soul, faculties, ready-made ideas or physiological processes. The temporary suspension of our consciousness is no objection to this view, Wundt thinks, provided a connection can be made out across the gap. How he would deal with double and split-off conscious personalities he does not indicate. This psychic causality is directly and intuitively perceived by self-observation, and thereby differs from physical causality, which is purely hypothetical and conceptual. It is here that the fundamental opposition between Wundt and the opposite school, who hold ultimately of Hume, appears. They would deny that we perceive in the sequence of our thoughts a constraining causal force any more than in the communication of force from one billiard ball to another.

With the principle of creative synthesis we are already familiar as the main idea of the whole book. The elements used by mechanical philosophers and psychologists in their constructions lack the qualities and the values of the real world of our experience. These qualities cannot be got out of the elements unless we grant the creative activity of the mind at each stage of the process. This principle, asserted against the Epicureans by the ancients, is a conclusive refutation of all attempts, from Lucretius to Herbert Spencer, to evolve the heterogeneous out of the homogeneous. Wundt is then sound in his main contention, as he is in affirming that the task of psychology is to trace the operation of this creative synthesis, rather than to elaborate unverifiable conjectures with regard to its physical parallels. What I cannot comprehend is his denial of the physical parallelism. Suppose, for argument's sake, that our analysis has reached elementary units. If these units are both psychic and physical, the psychic element has its physical parallel, which yet in no way accounts for its *quale*. Why, then, in the same sense, may not combinations of the one set of elements correspond in all cases to combinations of the other, though not explaining the specific quality that attaches to the compound? Will he say that it is because the only conceivable combination of the physical elements is spatial and temporal, and space and time themselves

exist only as products of the psychic syntheses, and imply a pre-existing consciousness capable of forming such syntheses? But is not this falling back into that idealistic denial of 'objects' from which we were told psychology (like the other sciences) must provisionally make abstraction? And does it not require for its explanation a complete statement of the idealistic philosophy of Wundt? And as I said in the beginning, can we be sure that it means anything very different from Münsterberg's admission that the fact of consciousness, as a whole, has no counterpart in the physical world? Thus while we all agree in deprecating the contamination of psychology with metaphysics, psychological literature is largely occupied with controversy over metaphysical conceptions introduced by the back door.

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ATTENTION AS INTENSIFYING SENSATION.

Dr. Münsterberg begins a paper on this subject (PSYCHOLOGICAL REVIEW, vol. 1, p. 39) by stating that the popular and generally received view is that attention does intensify our sensations. Common introspection certainly avers that attention as sensing effort generally, and within certain limits, is rewarded by increase of sensation. If I wish to hear better I listen harder; that is, I raise the sensation to stronger intensity by attending. I look for a dim star, I find it, and, increasing my cognitive effort, it appears brighter up to a certain maximum dependent on my state of health, training, etc. The keenest, most effortful glance gets the strongest sensation of a given light stimulus.

Fechner's observation that gray paper does not appear lighter the harder we look at it, does not destroy the fact that the more intensely we sense gray, the stronger is our sensation of it. And it must also be said that a dark object may with greater attention be discerned as gray, and dark gray as light gray; that is, where attention means not closeness of scrutiny, which often tends to close the eyes, but a wide open-eyed attempt to get full impression.

Further, the general theory of evolution leads us to suppose that only by attention has sensing and perceiving arisen and been developed in the struggle of life. It is by trying hard that the animal sees, and the harder it tries the more intensely it sees. Originally, then, a sensation becomes intense by attention as intensifying act, the sensing act is achieved and developed to various intensities only by and as cognitive effort. How is it, then, that an intense sound

gives us involuntarily, without the least effort, an intense sensation? This, we answer, is due to the efforts of ancestors for ages who at first were unable to hear the loudest sounds, but gradually achieved the hearing them, and hearing them intensely, and this tendency, perfected and integrated as useful in the struggle of existence, has been transmitted to us, in whom it acts automatically.

We believe, then, that if struggle or *nisus* is the fundamental power in the evolution of consciousness, then consciousness intensities of all kinds must be traced to attention; and so in the evolution of sense, attention is practically synonymous with sensation intensity. This does not deny that in certain cases of thoroughly integrated and automatic sensation cognitive volition when applied may be a hindrance, may reduce intensity and effectiveness of the sensation which always appear merely as a *given*. But this is of minor moment in a general discussion of attention. That we hear, and hear intensely, without listening, is because our innumerable ancestors listened, and listened hard. And so if future generations are to have certain forms and intensities of sensation *come* to them, it will be by our attention.

It may be evident, but it deserves emphasis, that we do not mean by sensation-attention, attending *to* a sensation. It is sufficient to know that where we now have intense sensations without intense attention, and without attention at all, this is not original and natural method, and that even now in general, when we wish to intensify our sensations, we exert cognitive effort, and with success. Expressed physiologically, it is the doctrine that function determines organ; that we see, not because we have eyes, but we have eyes because we see. The sensing effort has developed the eye, and by visual effort we now open the eyes wide and accommodate them, etc., thus securing intensity of sensation. Attending to a sensation is weakening to the sensation attended to. Thus, when absorbed in listening to music, some one asks me, 'Do you hear that false note?' the attention to the sensation as such weakens or destroys the sensation. Sensation-attention is not for us a consciousness outside of and directed to sensation, but sensing activity itself as cognitive effort. Nor is attention, as Mr. Shand implies, (*Mind*, Oct., 1894) a 'letting alone,' an isolating to see if a psychosis will strengthen, or will weaken and disappear. This hereditary spontaneous force of a cognition is the integrated result of past attentions, but is itself very different from attention as cognitive effort.

Believing, then, that sensation intensities are bound up with attention intensities as a general fact of mind, we were interested to

see how Dr. Münsterberg's experiments would bear on this law, to which he alludes in his opening remarks as the scope of his inquiry. However, we discover that it is only a certain kind of attention, expectant, and a certain kind of this, too much expectancy, that is really treated, with the result that sensations of light, sound, etc., are rendered less intense when we set our attention at too high a notch. The familiar experience of lifting falsely estimated weights is appealed to in a general way, but let us particularize. I see a two-pound wooden ball, which I take to be a ten-pound iron ball, and making muscular pre-adjustment, according to my misjudgment, it lifts 'as light as a feather;' I do not get the impression of a two-pound ball lifted with more just preparation or in a mere casual way. Thus sensations of weight may in intensity be inversely relative to the effort put forth. So if I am bid to look for a bright light or listen for a loud sound, and only slight stimuli actually occur, the sensations will be actually slighter in intensity than they would otherwise have been; the light does not seem so bright nor the sound so loud as when no pre-adjustment has been made. Now this result is not really 'unexpected,' but is quite the 'popular view.' We all know the answer which is commonly returned to those who realize certain sensations rather feebly, 'you set your hopes too high.'

But pre-adjustment may be too little as well as too much—a fact which Dr. Münsterberg does not notice—and the consequence is an undue increasing of intensity of sensation. When coming down stairs, and inadvertently taking two steps at a time, you have a peculiar abnormal increase of intensity of sensation, seeming to drop a very long distance, altogether disproportional to the actual distance, and this result is plainly owing to the wrong degree of pre-adjustment. Similarly for sensations of light, sound, etc., it is a common experience that under-adjustment means over-intensifying sensation. It is only when pre-conception and pre-adjustment are in a certain exact relation to stimuli that sensation occurs without abnormal heightening or lowering. It is well known that reaction-time is lessened by correct expectant attention.

The conditions and methods of the experiments call for some criticism. The agents were directed to have their attention 'fully' occupied with adding numbers. Now, adding is a process which is with most educated persons more or less automatic unless at top speed. But what is 'full' attention? Is it a scientifically determinable state, and one which can be induced as readily as securing air full of moisture at what we term saturation point? How can the experimenter be sure of attention at a certain degree? The inexact-

ness of experimental psychics as compared with physics is certainly great. The intensity of cognitive effort is neither easily discernible or measurable. However, we may say this, that attention at its strongest, is complete absorption of psychic capacity, and an agent in this state as regards adding effort would be entirely insensible to any stimuli. Further, full attention can only be reached by the full interest. When life for a wrecked sailor hangs on his seeing a certain beacon, then the intense interest is secured which assures intensest attention. But having, if possible, secured the highest attentions of several individuals, these attentions cannot be lumped together as identical in value. The tensile strength of iron bars of a given quality may be determined as equal, and the results used as a general value; but attention, as all mentality, has an individual equation. Again, it may be that the method of measuring intensity of sensation employed is the only feasible one, namely, judging intensity by the estimation the subject puts upon intensity of stimulus, still its inexactness is obvious. If a man says after lifting two rocks, one is twice as heavy as the other, are we thereby certain that one sensation was twice the intensity of the other, or may not the man judge also from other methods? At least the method ought not to be assumed without criticism and validation.

Further, Dr. Münsterberg finally explains attention as reducing intensity of sensation by feeling of strain. A tenseness of attention introduces a feeling of strain, which decreases the sensation felt. Sense of strain is undoubtedly divisive of consciousness, if with Dr. Münsterberg we interpret it as feeling of intensity. However, we do not gain anything by considering intensity of feeling as feeling of intensity. I have an intenser light sensation from the sun than from a candle, but if I say the intenser sensation is such by virtue of the greater feelings of tension, this means no more than that intensity of the sensation depends upon a sensation of an intense sensation, which latter intensity has to be explained, and so on. However, as I have pointed out, (*Mind*, XIV., p. 538) it is desirable to understand intensity not as a consciousness, but, like duration, as a quality. Every consciousness, including consciousness of intensity, has its intensity, which may or may not be felt or attended to. Strictly speaking, the intensity of my sensation—a feeling intensely—is never feeling of intensity, consciousness of intensity.

Still further, the 'chosen graduation of the stimuli' must be justified in the light of Weber's law, and the time intervals must also have their justification. The relation of change in time in attention and inattention and unattention to the problem of sensation inten-

sity must also be investigated if any satisfactory result is to be obtained. To get exact results in experimental psychology in general is then, I am persuaded, an enormously difficult task. The complexity of adult human consciousness is so great that it seems well nigh impossible to isolate the factors we are studying, and to secure identical reactions in a sufficiently large number of cases to prove a psychic law. Physical conditions are far more under our control than psychical, and are far easier to observe, and hence physical science has arrived at a *consensus* which is notably lacking in psychical. Essential preliminaries must first be settled before experimental psychology can really be fruitful, and the relation of attention to intensity of sensation requires far closer definition of subject and method than has yet been given it, if results of large scientific value are to be obtained.

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PLEASURE-PAIN AND EMOTION.

Serious and courteous criticism from the pen of a thinker, skilled in the subject of discussion, is certainly in all cases to be welcomed by an author, and I feel much gratification in reading Dr. Santayana's remarks upon my lately-published book[1] in the July number of this REVIEW.

There are one or two points raised in the review which I think it worth while to discuss.

In the first place, in the interest of psychological advance I must deprecate the implication of the opening paragraphs; viz.: that the writer of what aims to be a scientific discussion of pyschologic doctrine is no great sinner if he consider the claims of literary æsthetics in his exposition, where there is the slightest chance that the clearness and definiteness of his meaning may thereby suffer.

I regret much more than my critic can do that the book is so unattractive in its literary quality, but on the whole I do not feel confident that I could have made it more pleasing had I not deemed it of the utmost importance to aim at accuracy and to waive verbal preferences in favor of precision.

I am free to confess that the reading over and over again of my proofs has produced within me a deep-seated digust with many phrasess in the book, notably with the compound word *pleasure-pain*; but what authorized substitute could I have used in this case save the word

[1] Pain, Pleasure and Æsthetics.

'feeling'?—a word which is truly much more euphonious than the one employed, but entirely devoid of accuracy. I do not wish to excuse the evil complained of by my critic, a cleverer writer might have overcome it; but I think that it would have been all wrong to have chosen in any case literary worth as against definiteness, in such a work.

I raise this point principally because I feel that psychologists to-day are too often careless in this regard. They are too apt to discard in disgust an awkward but accurate term or phrase and to use in its place something of better æsthetic quality but decidedly inferior in definiteness. Or they go even further and add emphasis to unimportant particulars by the attractive nature of some form of speech or of some chance illustration. The extraordinary misconception of Prof. James' emotional theory by other psychologists, to which he draws attention in the September number of this REVIEW, may in my opinion be partly accounted for in this way. I have in mind a case in which I myself entirely lost the drift of an interesting argument presented in a paper read before the last meeting of the Psychological Association, because my mind refused to be dragged away from the æsthetic contemplation of a happily-used and beautiful quotation to the hard thinking required in following the course of the argument.

In the field which I touch the preference of euphonious but inaccurate terms and phrases, where 'barbarous' but accurate ones could be found, has been especially unfortunate in result.

I am very sure, for instance, that much of the voluminous literature of the subject of which I have treated would have remained unpublished had the authors avoided the use of '*Gefühl*' in German and '*Feeling*' in English. Had they used 'pleasure-pain' (or some better equivalent) when and only when they meant it and nothing else, many of their most effective periods would have become evidently illogical or irrelevant. My critic shall furnish me with an example of the danger. It is much pleasanter to speak of the æsthetic as determined by pleasures of *memory*, than by pleasures of *revival*; and to avoid repetition I did give way once, I believe, and use that term in one of the statedly popular summaries. But the pleasures of *memory* are not all that I refer to. A memory is a special kind of revival. *Revival* therefore is a much broader term than *memory*. What I refer to are *revival* pleasures, and the use of the word *memory* in this connection at once limits the thought of the reader to definite objects: With my critic, the notion that the two

terms are interchangeable has led him at times to misconceive to some extent the thesis presented for examination.

What has worried me indeed has been not so much the failures of style, to which Dr. Santayana calls attention, as the consciousness that I may possibly have been guilty of the very faults of inaccuracy that I deplore in others.

But to turn to the criticism itself, I have no desire to combat objections raised except where they seem to involve misapprehension, and I am glad to say that I think all of Dr. Santayana's oppositions, as expressed, will disappear upon a clearer apprehension of my meaning.

I hold that there is no "clear distinction between the sense of pleasure and the sense of beauty" *in impression:* and these last two words, that I add, are of the very essence of my thesis. The distinction which is noticed is one made in judgment upon revivals. With these two words added to Dr. Santayana's expression I have no hesitation in leaving the cases he brings forward to introspective tests. I am sure that for myself when "I have no definite object before the mind, but am lost in a torpid reverie" which is pleasant, the state of *impression* is indistinguishable from many of the impressions that are *called* distinctly æsthetic; *e. g.*, the impressions coming to me as I listen to some parts of Wagner's 'Tristan and Isolde'; and I have no hesitancy in holding that if an object *afterwards to be judged beautiful* were to appear in connection with this reverie, the pleasures of æsthetic *impression* connected with this object would completely fuse with those of the 'torpid reverie.' The point of difference lies just in the distinction between the æsthetic judgment and the æsthetic impression, the former of which always relates to objects or objective states. In this particular case the revival of the state of 'torpid reverie' is necessarily associated with the torpid object, and for most people such torpid objects or their mental states are, in revival, so very insipid that they cannot be noticed to be pleasurable and are therefore *judged* to be unæsthetic. Dr. Santayana says "but this pleasure" (of torpid reverie) "would not be æsthetic, because I could not perceive any beauty, seeing that no object is present to me in which that beauty may reside." By the words 'would not be æsthetic' he certainly means "would not be called or thought of in retrospect as æsthetic"; he is speaking of what I consider to be a judgment as to the nature of revivals. His words, however, would lead one to think that he considers this phrase to relate to the direct nature of the impression.

The other case mentioned by my critic is also, I think, distinctly in my favor. It is perfectly true that "few pleasures are so vivid in revival as those of satisfied vanity, affection, revenge and other personal passions," and so far as I have indulged myself in these intoxicants I feel sure that I have been æsthetically *impressed* at the time. I am unable to draw any distinction between these pleasurable *impressions*, so long as they remain mere *impressions* and those other *impressions* produced by what is acknowledged to be beautiful. The distinction comes in the revivals upon which we act in judgment, when the despicableness of the self-complacency brings a balance of pain to a man who is properly constituted. My critic's examples, indeed, are not here very forcible, for, in the revivals of the 'personal passions' mentioned, I am usually distinctly judging of myself as worthy in some respect and therefore as an æsthetic object.

There is another direction in which I wish to make my position clearer. I am one of those who think that too much emphasis is given to-day in some quarters to the physiological basis of psychology. I am heartily in sympathy with any investigations that can throw light upon psychology, and I think the patience and persistency of our experimenters in psychophysics is most noble, and, except so far as it is misapplied, it certainly should be most heartily encouraged and applauded. On the other hand, I feel with many of the advanced neurologists that we can only claim to be *beginning* to understand the nature of those neural changes which form the basis of psychic life. I do not feel sure that our present notions of the relation between mind and body may not seem very crude in a few centuries from now, just as those held by the Greek philosophers do to us to-day.

Just here it will be convenient and appropriate to call attention to a point which relates to this subject-matter, and which supports the view just expressed. Two years ago Dr. H. Nichols published in the *Philosophical Review* a defence of a theory that pains are a species of sensation. I argued in a reply, which appears again in my book, that this view is opposed to psychological evidence, and that the facts, mainly physiological and histological, upon which the theory depends for its support are, with possibly one exception, entirely compatible with other deductions than those made by those upholding the sensational theory. This one exception was the claim made that Goldscheider had discovered definite nerve-terminals for pain in the skin. I objected that in this field the statement of one observer of a limited number of subjects should be received with caution, and I further noted that Goldscheider had *implicitly* denied the

position involved in his first statement as it was interpreted, and upon which interpretation Dr. Nichols founded his argument. Dr. L. Witmer has lately reiterated Dr. Nichols' theory in the *Journal of Nervous and Mental Diseases* for April, 1894, and has sharply called me to task for being unwilling to accept as final Goldscheider's supposed dictum. Dr. Collins, in his review of my book in the same journal, has made the same criticism of my position in this respect. But now there comes to hand a new book by Goldscheider.—'*Ueber den Schmerz*,' Berlin, 1894,—which serves I think to teach a lesson to all psychologists, and especially to those who may have taken interest in this discussion; for in this book Goldscheider distinctly denies the view which has been thus attributed to him, and seems to think he cannot properly have been held to be a defender of a position so evidently untenable; although I think his *words* in his early publications certainly spoke clearly as they have been understood. Goldscheider now holds (p. 7) that *Schmerz* is "eine besondere Qualität der Empfindung, nicht eine allen verschiedenen Qualitäten gemeinschaftliche Modifikation der Empfindung." Further (p. 13), "dass die Schmerzempfindung den Drucksinn and Gemein-gefühlsnerven eigen ist, allen übrigen Sinnesnerven aber fehlt." I do not appreciate upon what sufficient grounds he bases his belief in the existence of these Gemeingefühlsnerven (see also p. 33) and of the Gemeingefühlserregungen spoken of elsewhere (see p. 8). He tells us further (p. 18), "Hiernach läge es in der That nahe, jeden Schmerz als ein Summations-Phänomen anzusehen, allein dies gilt nicht ausnahmslos": and he postulates a 'Summations-Organ' (see p. 34) located in the spinal cord (see p. 19) to account for the effects of pain. It does not seem to me that we should receive without caution the statements of an investigator who makes such free use of unverified hypotheses.

Goldscheider in this new treatise, if he does nothing else, shows conclusively that our knowledge of the nature of the neural changes which are the coincidents of pain consciousness is of the most indefinite character, open to dispute in every direction, and that no physiological or histological theory relating thereto can to-day be held to be proved. Moreover, so far as I can see, there is little reason to lead us to hope that we shall be able to reach any settled position in this respect in the near future.

This occurrence strengthens within me the conviction with which I wish more of our psychologists clearly showed their sympathy, that introspective psychology must move on in her development without waiting for the positive teaching of psycho-physics; she

must of course endeavor to check the results of introspection by what becomes known through psychological investigation ; but it would surely be a great loss to philosophy and to science in general if psychology hesitated in her course while awaiting clear light from this source.

This being my view, it was with regret that I found it impossible to discuss adequately in my book that which has been done in the past in reference to the physiological basis of pleasure and pain, and to suggest the direction in which the facts before us vaguely point, without giving relatively much more space to the subject than its importance warrants.

I am disappointed, moreover, to find notices of the physiological theory so prominent in the reviews of the book, and especially to find Dr. Santayana in this review taking for granted that the basis of my æsthetic principles is to be found only in this necessarily vague physiological theory. In fact if the reader will take the trouble to examine the matter he will find that the basis of these æsthetic distinctions and principles is really determined by introspective evidence and not derived from physiological hypotheses ; and that the physiological correspondence, as it would appear under my theory, is generally stated in the chapters dealing with these principles, in small print in brackets. Thus it appears indeed that the psychological æsthetic principles give us a very strong corroboration of the physiological theory, but I do not feel that it is evident that these æsthetic principles necessarily fall if the physical theory crumble into dust, as is implied in this review, and as has been asserted by Dr. Jos. Collins in the review in the *Journal of Nervous and Mental Diseases* above referred to.

It is because I feel the secondary importance of this physiological view that I am also greatly disappointed to find my critic holding that my main *psychological* thesis turns upon a mere matter of words. I had hoped to show that this psychological thesis has strength; and that, so far as our knowledge of the physiological aspect of the subject goes, the psychological view is not incompatible with that knowledge ; for I seem to see that if the psychological view be true, and if it be carried out to its consequences, it may lead to results in other directions than those especially studied that may prove to be interesting at least ; and I had hoped that the results as brought forward in relation to the emotions, the art impulse, and the principles of æsthetics, might appear to be not wholly valueless, altogether apart from any physiological theory whatever. As this psychological theory is in my view thus important, I trust that it will not appear

out of place if I try to convince my critic that it is a great deal more than a mere verbal contention that I make.

In the first place I may again call attention to the dangers attendant upon the æsthetic treatment of what should be strictly accurate science. Dr. Santayana's statement of my proposed definition of pleasure and pain is probably pleasanter to his ear than my own. He states that I hold them to be "qualities either of which may and one of which must belong to *every perception of the mind.*" But this is not the doctrine I have expressed, unless my phrase 'each element of consciousness' is made equivalent to his 'every perception of the mind.' I do not think the two expressions are at all synonymous, and I believe that one must avoid the statement as made by him if one is to grasp correctly the thesis and its implications.

But passing over this inaccuracy, let us turn to his argument itself. It is perfectly true that we may equally well say "that color is a quality of extension, or that they are two simultaneous perceptions, or that they are both qualities of a present substance." But is it no gain to take note of the fact that at times when there is no color perception at all there still may be a consciousness of extension; that other sensations and mental modes than color sensations have this consciousness of extension connected with them? Is no importance to be attached to the thesis that *extensity* may be a quality of very wide application? Can it justly be held that the contention of the present day in relation to space, which turns upon this thesis, and in which the greatest psychological thinkers of our time are involved,—that this contention 'turns upon a matter of words,' '*becomes real and not verbal only in the field of physiology*'?

Or to take another instance more to my liking. The *intensity* of a color may be treated as Dr. Santayana treats its extensity. It might be said that the facts may be "described equally well by saying that color is a quality of" intensity, "or that they are two simultaneous perceptions, or that they are both qualities of a present substance." But is it no gain to psychology that more or less of intensity is acknowledged to be a quality attached to all elements of consciousness? Would it be of no value to make contention for this doctrine were psychology in so crude a state that some masters held 'intensity' to be a species of sensation; others that it is a kind of emotion; others that it is the fundamental basis of all psychic life; others of great weight that a special kind of mind, apart from our cognitive mind, must be postulated to enable us to grasp *intensity* —*i. e.*, that *intensity* is a mental mode *sui generis*? Could such con-

tention, if it were necessary, be held to turn upon a mere matter of words, and to become a real question 'and not verbal only in the field of physiology'? It is such a contention as this that I make for pleasure-pain.

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A COMMENT.

In Wundt's article, *Zur Lehre von den Gemüthsbewegungen* (Phil. Stud., vi, p. 364), occurs the following passage:

Die Apperception selbst ist nichts, was den Effecten, die sie am Vorstellungsinhalte erzeugt, und den Begleiterscheinungen, die sie im Gebiet des Gefühls hat, als etwas besonderes, realiter zu trennendes gegenüberstände. Vielmehr besteht sie selbst nur aus diesen Begleiterscheinungen und Wirkungen.

Professor James exclaims, àpropos of the last sentence: "A thing that 'consists' of its concomitants!" (This REVIEW, I, p. 516.) The exclamation is hardly fair criticism. We read, p. 390 of the same article:

Zu jenen Begleiterscheinungen reche ich in erster Linie gewisse zu Vorstellungen vereinigte Empfindungen, in zweiter Linie die Willensacte theils vorbereitenden theils mit ihnen unmittelbar verbundenen Gefühle. Die letzteren lassen sich jedoch nur auf Grund der einmal vollzogenen abstracten Unterscheidung zwischen Fühlen und Wollen Begleiterscheinungen des Willens nennen. In Zusammenhang mit der Entwickelung des Willens betrachtet, verwandeln sie sich selbst in Elemente der Willensthätigkeit, die sich aber deshalb, weil aus ihnen nicht immer ein actuelles Wollen hervorgeht, nun auch in solchen Fällen, wo dieses eintritt, demselben als begrifflich trennbare Bestandtheile gegenüberstellen lassen.

It is, of course, really not much fairer to quote two passages without context than it is to quote one. But the second of the above citations may serve to show that Professor James' scorn is not so undoubtedly merited as might at first sight appear.

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PSYCHOLOGICAL LITERATURE.

HALLUCINATIONS AND TELEPATHY.

Ueber die Trugwahrnehmung (Hallucination und Illusion) mit besonderer Berücksichtigung der internationalen enquête über Wachhallucination bei Gesunden. EDMUND PARISH. Leipzig, Abel, 1894, [Schriften d. Ges. f. psych. Forschung, Heft 7–8; II. Sammlung] Pp. 246.

The erudition of Herr Parish's work is exemplary and admirable, and in its text and footnotes it is safe to say that one may find reference to everything, important and unimportant, that in recent years has been written on hallucinations from either the medical or the psychological point of view. The author's personal contributions to the subject are animated by the laudable desire to minimize mysteries and to explain the exceptional phenomena of which he treats by the laws of ordininary mental life. The important points in the book are, first, Herr Parish's general theory of the hallucinatory process, a theory which he applies to all possible cases; and second, his verdict of *non liquet* upon the telepathic theory of veridical hallucinations maintained by the English 'psychical researchers.'

His theory of the hallucinatory process is that it is always an incident of 'dissociated' conditions of consciousness. By a dissociated condition he means one in which ordinary channels of association are obstructed. Reviewing the conditions under which hallucination is apt to occur, he finds them predominantly to be of this sort. In sleep, in the borderland between sleeping and waking, in melancholy, in hysteria, epilepsy, the delirum of fever, of fasting, and of certain narcotic poisonings, in hypnotism and crystal gazing, the fact of obstructed associations is admitted by all. Even in mania and drunkenness, where association seems at first sight rampant enough, this is chiefly verbal association, and objective thought is enfeebled and slow. The way in which dissociation facilitates hallucination is according to Herr P., this[1]: A stimulus is always drafted off into the most pervious paths at the time being. In normal association

[1] Herr P. expressly bases his theory on that of the hallucinatory process given in James' Principles of Psychology, II. ii.ff.

these are the most habitual paths. But there are always many stimuli at work, and many 'cerebrostatical' conditions determining perviousness, so that the final process aroused by a stimulus is the result of an intricate array of factors. Whatever path is followed *to a pause*, gives there a vivid sensible content which, in normal cases, involves a veracious perception of the object from which the stimulus comes. But if at any moment a dissociative condition is realized, so that the usual paths are blocked, whilst at the same moment other accidental paths are in a state of exalted tension from inner causes, then into these latter the stimulus discharges its energy, making them explode with the maximum of force; so that the result is the perception of an object having no usual connection with the stimulus, and by the vividness of which the consciousness of the latter may be eclipsed. The reigning state of obstructed association moreover weakens the subject's critical reaction, and the false perception is not only experienced but believed. This theory is ably defended by our author, and has the merit of being very general, and of bringing hallucinations and illusions under a common law.

Do the sporadic waking hallucinations inquired into by the 'Census' of the International Congress of Psychologists easily fit under this law? Our author tries to make them do so. First he attacks the truth of the Census, in which 'borderland' cases are hardly more than half as numerous as the 'waking' cases. Considering this to be *a priori* impossible, he explains the actual statistics plausibly enough by the greater tendency of the borderland cases to be forgotten (it being already demonstrated that the majority of all hallucinations *are* forgotten). Next, taking the alleged waking cases, he shows by a number of examples that in them also dreaminess or some other dissociated consciousness may be supposed—The Subject was 'fixating' something, if no stronger reason can be alleged. I must say that Herr Parish seems to me here to drive his theory a little too hard. Many of the narratives so distinctly belong to normal consciousness, that the better tactics would be to discredit their veracity altogether; and this method also, Herr Parish applies vigorously to the particular class of hallucinations called veridical or coincidental (*e. g.*, with the death of the person perceived).

Prof. Royce's suggestion that the narratives are often due to 'pseudo-presentiment' (false belief, after the death has happened, that it had been symbolized by an apparition previously) is made liberal use of, in spite of its almost absolutely conjectural character. The much sounder objection follows that genuinely occurring hallu-

cinations are equipped afterwards, by the retrospective imagination of their percipients, with details that fit those of the event with which, when it happens, they are supposed to be connected. This especially applies to them where they are collective, the different percipients obeying each other's suggestion as to what they saw. Finally the false appearance of frequency of hallucinations of the coincidental class is explained by the far greater tendency of the non-coincidentals to become forgotten, the coincidentals resisting oblivion. Furthermore, Herr Parish contends that the 'frequency' of the coincidentals should in any discussion as to their being due to chance be set down as the ratio of their number to that of hallucinations of all varieties, and not to that of their own variety, which in the argument of the English committee is defined as that of 'apparitions of recognized living persons.' For all these reasons, Herr Parish concludes, the alleged frequency of the veridical class of hallucinations becomes so reduced as to form no argument against the genuine cases among them being due to chance. Moreover, he adds, we cannot lump the cases in one order of probability. Where for example the percipient is the anxious child of an aged parent ill with pneumonia, the chances are that if she have an hallucination at all, it will have that parent for its subject.

Herr Parish's criticisms are partly based on the provisional report of the English committee published at the International Congress of 1892. The committee have themselves considered such objections in their final report, which forms the subject of our next article. So I will immediately proceed to give some account of that. I will say meanwhile that this German critic's tone is uniformly respectful; that he himself prints the 59-yes cases of the Munich Census, of which 11 are more or less coincidental; and finally that his work is the most solid existing contribution to the subject up to the date of the report whose title follows below.

Apparitions and Thought Transference, an Examination of the Evidence for Telepathy. FRANK PODMORE. Contemporary Scientific Series. New York, Charles Scribner's Sons, 1894. 12°, pp. 401.

Mr. Podmore gives here a convenient summary of the work of the Society for Psychical Research, striving to make the theory of telepathy cover as much of the field as it can be stretched over. When one sees brought together, as here in the early chapters, the evidence for thought-transference drawn from the simple experiment in which one person is set to guessing numbers, drawings, etc., which another person is intently looking-at or thinking-of, one perceives

that it is far from contemptible in either quality or amount, and even if one is unwilling oneself to follow, one can find no very harsh names to apply to those who, like Mr. Podmore, take thought-transference as an approved *vera causa*, and try by its means to explain such phenomena as apparitions at the time of death, distinct in nature as they appear at first sight to be from the successful guessing of pictures in another's mind.

The book mentions successful experiments of the simple order with at least thirty subjects at short-range, and this leaves out many of the records published in the S. P. R. Proceedings. Of course these experiments are of diverse value, some of them being too brief or too faulty in method to base strict conclusions on, but they all contribute to the cumulative impression that chance and trickery can with difficulty be supposed to be the only things concerned. As an instance of a good series I take the observations of Mrs. Sidgwick on five hypnotized subjects who guessed numbers drawn by a third person from a bag containing 81 lotto-counters marked from 10 to 90, and handed to the hypnotizer to gaze at, all this of course out of sight of the subject. Out of 644 trials 131 were successful, that is, both digits were given correctly, though in 14 out of the 131 cases the order was reversed. 'Chance' should only have given 8 correct guesses. Again, with hypnotizer and subject in different rooms, there were 27 quite correct guesses, instead of the chance-number, 3. In the unsuccessful trials here, the first digit came right 85 out of the 252 times, instead of the chance number, 28. Mrs. Sidgwick went through another series with the same subjects, in which 'mental pictures' were the things to be guessed, some of them being quite complex scenes, in all 108 experiments, of which 33 were correct. Of these trials, 55 were made with the agent and percipient in different rooms, so that the successes in the same room were 31 out of 71. Practically, since collusion seems fairly excluded, the only recourse of the doubter here is to say that the series were too short and that farther experimentation would have reduced the success to the chance-number. And here is where the force of so many other successful series, longer or shorter, comes in. They make the reader feel as if the dice must be in some way loaded; and to the force that loads them Mr. Podmore and his colleagues have given a name, that, namely, of *telepathy*, in lieu of a theory about it.—It is clear that many series of guesses with more successes than the probability due to chance can yield, will not positively prove that chance may not have produced the result after all; and it is still clearer that such statistics are no guide as to what the

positive force may be. And here the other phenomena gone over by Mr. Podmore come in to give some feeble help. But they run into a mass of details ill adapted for synopsis, so with this brief notice I conclude.

Report on the Census of Hallucinations. H. SIDGWICK, A. JOHNSON, F. W. H. MYERS, F. PODMORE, E. M. SIDGWICK. Proceedings of the Society of Psychical Research. Part XXVI. Aug., 1894. Vol. X., pp. 25–422.

This extraordinarily thorough and accurate piece of work is understood to be the fruit mainly of Mrs. Sidgwick's labors; and the present reviewer, who has had a little experience of his own with the 'Census,' and knows something of its difficulties, may be allowed to pay his tribute of admiration to the energy and skill with which that lady and the other members of the committee have executed their burdensome task. They collected no fewer than 17,000 answers to the question: have you had, when awake, etc., an hallucination, etc. Of these answers 2,272 were 'yes,' and these Yes-cases were corresponded with or interviewed or in other ways subjected to as critical a scrutiny as circumstances allowed. The result is an unusually careful handling of the raw material offered, and a great accession of new facts. The census of hallucination was, as is well known, an idea of the late Edmund Gurney, who thought that the theory of chance-coincidence applied to 'apparitions' reported as occurring on the day of death of the person appearing might be tested by statistics. Gurney himself collected 5705 answers, and, applying statistical reasoning to them, thought it superabundantly proved that the 'veridical' cases amongst them were too frequent to be due to chance. The Sidgwick report, unlike that of Herr Parish, keeps the Gurney question well to the front, and its general discussion of the physiological and other conditions of the hallucinatory process is less erudite and elaborate than that of the German writer.

I will quote immediately the conclusions of the report as to apparitions at the time of death. "We have 30 death-coincidences in 1300 cases [of visual hallucination of recognized living persons] or about 1 in 43. But chance would . . . . produce death-coincidences at the rate of 1 in 19,000 apparitions of recognized living persons, and 1 in 43 is equivalent to about 440 in 19,000, or 440 times the most probable number. Or, looking at the matter in a different way, we should expect that if death-coincidences only occur by chance, it will require 30 times 19,000, or 570,000 apparitions of living persons to produce 30 such coincidences. . . . . We con-

clude then that the number of death-coincidences in our collection, if our estimate of them is accepted as fair, is not due to chance. This will not be maintained by anyone with the most elementary acquaintance with the doctrine of chances. The opponent of a telepathic or other supernormal explanation must take one of three other lines of argument, . . . even one death-coincidence being more than we should be justified in expecting chance to produce in a collection ten times the size of ours" (p. 247–8).

Everything in this conclusion depends on the numerical premises being severally reached in legitimate ways.

In the first place, take the assumption that out of 19,000 apparitions of the sort considered, only 1 should be expected to occur on the day of death of the person seen. This is based on the mean death-rate of England. Since in England the mean annual death-rate at present is 19.15 per 1,000 of population, the mean daily death-rate must be 365 times less, or 1 in about 19,000. All daily operations concerning persons, if not directly contingent upon their death, would under these conditions be more likely to strike the living than the dying in the proportion of 19,000 to 1, and this no matter how frequent or infrequent absolutely such operations should prove to be. Apparitions are operations concerning persons; and whether such apparitions be as frequent as dreams, or whether they be very rare, whether a large fraction or a small fraction of the population be visited by them, we should expect (if they be due to mere chance) always to find this proportion observed, that only $\frac{1}{19000}$ of them should be of people who were dying on the day when their apparition took place. [This 'day' is measured in the report by the 12 hours preceding and the 12 hours following the death.] To the present writer this reasoning and computation seem valid.[1]

[1] In particular does the contention of Herr Parish (see the article on him, above, *ad finem*) seems inadmissible. He says that in estimating the probability that apparitions at the time of death are due to something more than chance we ought to measure their frequency by the ratio of their number to that of *the aggregate of all phantasms of whatsoever description*. He would even include illusions, since the process of illusion and hallucination are for him fundamentally the same. To base an argument on the ratio between the number of veridical death-apparitions and that of merely *all apparitions of recognized living persons*, he says, is a *petitio principii*. The point is a subtle one, and may well make one momentarily hesitate, but reflection leaves no permanent doubt. We have three orders of frequency in hallucinations to consider, that of hallucinations at large, that of hallucinations of persons, and that of hallucinations of dying persons. These may be caused by their respective objects, or may come at 'random,' their causes lying exclusively in the subjective cycle. The point is to see whether anything in the frequency itself can help us to decide which of these alternatives is the true one. Now with what frequency in outer things might these frequencies

Next, how are the numbers 1,300, for the whole number of visual apparitions of recognized living persons, and 30 for the coincidental ones among them, established? Neither of these numbers is that of the crude face of the census-returns, each being a number estimated by applying certain corrections to those returns, the corrections all being such as to weight the figures in favor of chance-coincidence as far as this can with any plausibility be done. The crude returns certainly include an unduly large percentage of coincidental apparitions, partly because a large number of non-coincidental ones are speedily forgotten and do not figure in the returns, and partly because, of the coincidental ones, some are likely to have been put in by careless collectors on account of that character, and not to have simply turned up in the census-taking by due process of chance. Now can any definite estimate be made of the amount of error that has crept into the census from these sources? The authors of the report find, by comparing the dates of the returns, that cases are the more frequent the more recent they are. This proves a forgetfulness increasing with antiquity. The obvious remedy would be, ascertaining what recent period could be taken as trustworthy, to find out how many hallucinations had visited the persons figuring in the census during that time, and then to treat

in hallucinations keep tally in the two cases, of outer causation and of no outer causation respectively? Obviously if persons do not cause hallucinations of themselves, the hallucinations of persons should be *no more* frequent among hallucinations than persons are frequent among all the things that may become objects of hallucinations; whilst on the contrary, if persons, and persons alone, do cause hallucinations, then hallucinations of persons *should* be relatively more frequent than other hallucinations, because the causation by the real outer object would be simply added, for this class alone, to the random inner causes that produce hallucinations in general. Similarly if the deaths of persons do not tend to cause hallucinations of those persons, the hallucinations of the dying should be *no more* frequent among hallucinations of persons than the dying themselves are frequent among persons; whilst if on the contrary the dying, and the dying alone among persons, do cause hallucinations of themselves, then these hallucinations should be more frequent among hallucinations of persons than the dying are among the whole population of persons. This latter ratio is what the Sidgwick committee finds realized in fact; hence its conclusion that the dying do cause hallucinations of themselves. Herr Parish's selection of the total number of hallucinations *überhaupt* as one subjective term of comparison leads to a statistical test which is also true in theory, *provided the corresponding objective terms be altered to match.* We shall then have (if dying persons do *not* cause hallucinations of themselves) this proportion: As is the ratio of real dying persons to all other real things, so at its highest should be the ratio of hallucinations of the dying to all other hallucinations whatsoever. But although there is no theoretic objection to this proportion, it is practically worthless, because we have no statistical data by which to compute the ratio of dying persons to all other real things.

the earlier part of their lives as if, in spite of their yielding smaller 'returns,' they must really have included as large a number, proportionally, of similar experiences. Taking the past 3 months as the trustworthy period, and considering visual cases alone, the authors of the report agree that the face-returns should be multiplied by 4, in order to represent the true number of 'apparitions' seen by their informants. But, as the total number of specifically described apparitions of recognized living persons returned in the census equals 350, and $350 \times 4 = 1{,}400$, the round number of 1,300 may be taken as probably near the figure sought.[1]

The whole number of death-coincidences amongst the 350 cases in question is 65, or 62 when 3 cases known to be selected by their collectors are struck out. There is no ground for supposing that death-coincidences tend to be forgotten by their percipients : On the contrary the cases appearing in the census date with disproportionate frequency from by-gone decades. This, of course, may be due to the fact that the number 62 is too small to give true averages when distributed over the 36 years covered. But to be on the side of severity the committee assume that the proportion reported from the last decade is the only normal one, and that the earlier stories may be false, and (by a computation based on figures which need not here be reproduced) they knock off 22 on this account from the total of death-apparitions to be used, and make it 40 instead of 62, just the opposite treatment to that which they applied to the gross group of 350 cases of which these death-cases are a part. From these 40 they again knock off 8 as an ample allowance for possibly unreported selection on the collector's part[2], and again 2 for good measure and as a sop to the adversary, so that finally the reduced number of 'veridicals' to be compared with the augmented number of veridicals and non-veridicals taken together, falls to the figure 30 which is used in the conclusion quoted from the report on a previous page.

[1] The period of three months is found trustworthy when 'suspicious' cases are eliminated. Suspicious cases are those where the appearance may not have been an hallucination. Figures seen in a bad light, or through an open door in passing, or at a distance in the open air, are included in this category. Study of the cases reported to have occurred within three months of the accounts given, shows that these 'suspicious' ones are rarest in the first month, and are therefore presumably peculiarly liable to oblivescence. But if they are counted in, one month and not three months becomes the trustworthy period, and the multiplier of the crude returns must then be changed from 4 to 6½. The influence of this counting of suspicious cases is considerably to enlarge the total of hallucinations to be supposed, and to make the odds in favor of the coincidental ones being due to something else than chance sink from 440 to 292 against 1.

[2] The data for computing this number of 8 are given on p. 243 of the report.

The reader will appreciate the candor of the committee, and see how earnestly they have sought to eliminate all that might add specious color, as distinguished from real weight, to their own side. The reader whom their argument does not impress will have, they say, to take one of three courses. He may deny the accuracy of the coincidental cases, to which the reply of the committee consists in printing 31 good ones as a sample. He may still insist that the collectors have loaded their returns with an excessive number of these cases, to which the reply is too minute for quotation here (pp. 57 and 210 of the Report) but amounts to a detailed proof that there is probably no overloading of the returns in general with *yeses*, and to good reason shown for the opinion that of the 62 coincidental apparitions taken as a basis for the enquiry, at most 10 can be assumed as possibly added deliberately by the collectors to their returns. But these have been eliminated in the reduced number of 30, finally admitted to count in the argument.—Thirdly the objector may say that many of the veridical apparitions *are* causally connected with the death, but not by telepathy or any other *vis occulta*. The illness of an aged person is the cause both of death and of anxiety among relatives. Anxiety is proved by the committee's own facts to predispose to hallucination[1]; so both the hallucination in such cases and the death can be common effects of a single natural cause, the illness, working on two persons. This, it will be remembered, is Parish's final objection, mentioned above; and the report treats it as important. At the same time the authors point out that there are but 23 cases of the 62 veridicals in which the illness was known beforehand, and only in some of these was there anxiety. Moreover the close coincidence *in hour* of the death with the apparition in so many cases seems to preclude the application on a large scale of a cause like anxiety which in the nature of things must have lasted many hours or days.[2]

[1] Anxiety about illness was probably present in 89 out of the 1622 cases of which there are first-hand accounts, and grief about death in 42 of the other cases, making nearly 1-12 of the whole number. As we don't spend 1-12 of our lives in grief and anxiety of these sorts it must be that during these emotions hallucinations come with undue frequency.

[2] Mere expectation, which often causes illusions, seems to play no important part in causing hallucinations. At least the committee find only 14 cases in the whole collection where the phantasm was of a person for whose arrival the percipient was looking out. They give cases where 'suggestion' may be reckoned a cause (collective cases, prediction of apparition at spiritist seance, etc.), but these are ambiguous, and if occult agency be once admitted as a possibility, are perhaps as likely to be caused by that as by 'suggestion'.

It will thus be seen that the committee have considered on their own account all the difficulties urged by Herr Parish (with the exception of the 'pseudo-presentiment' hypothesis of Royce) and that they have considered them in a more objective and less conjectural way than he, without their case being weakened to any certain extent.[1] Plainly, though, if the 30 cases left to be used in the argument could all have been first-class cases (with record of hallucination before event, no anxiety, etc.) the argument would have been more convincing. But the successive weedings of the crude number 62 could not be performed selectively so as to accomplish just this result, and the Census is therefore still too small for *knock-down* proof of occult cause. If telepathy be regarded on other grounds as possible, then these statistics make it extremely probable. Otherwise they will not convert the disbeliever, who will pooh-pooh the statistical method *in toto* when it takes 17,000 answers to get 30 good cases to cipher with, saying that the field is too vast and lean for profitable reaping, that figures got by applying so many hypothetical corrections to inaccurate crude data, savor too much of guess-work to inspire confidence, and that cooked returns are cooked returns, even though, like these, they be cooked for the safe side, the side adverse to the conclusion reached by their means.[2]

This sort of reception by the hard-hearted is inevitable, and it is useless to ask how strictly logical it may be, for belief follows psychological and not logical laws. A single veridical hallucination experienced by one's self or by some friend who tells one all the circumstances has more influence over the mind than the largest calculated numerical probability either for or against. I can testify to this from direct observation. The case will, therefore, still hang

[1] The only criticism I can make is that the committee have possibly been too indulgent to the cases where the percipient was in bed. His conviction that he was awake is to be taken with large allowance under these circumstances.

[2] The figure 4, for example, used as a multiplier of the crude returns in correction of forgetfulness, is reached by this process: out of 87 visual hallucinations reported for the most recent year, 42 are stated to have occurred within the most recent quarter, and of these 19 within the most recent month, and 12 within the most recent half-month; numbers which correspond approximately to 168, 228, and 288 per annum instead of 87. But if from the 87 the 'suspicious' cases as described above are eliminated, and the most recent quarter examined, the figures are much more even. There are 12 suspicious cases in the recent quarter; so that then 30 instead of 42 becomes the number to be counted in the quarter. Of these the last month shows 12, and the last half-month 5, numbers which correspond to 120, 144, and 120 per annum respectively. This looks like distribution by 'natural law,' *provided the evenness of the figures be not accidental.* But where such small numbers are involved, how can one be sure on that point?

pending before public opinion, in spite of the laborious industry of Mrs. Sidgwick and her colleagues. Of course if the results of the American Census, not yet published, should correspond, that will add retroactive weight. But the most that can be said, so far, in the opinion of the present writer, is this, that the Sidgwick report affords *a most formidable presumption* that veridical hallucinations are due to something more than chance. Now this means that the telepathic theory, and whatever other occult theories may offer themselves, have fairly conquered the right to a patient and respectful hearing before the scientific bar; and no one with any real conception of what the word 'Science' means, can fail to realize the profound issues which such a fact as this may involve.

WILLIAM JAMES.

ETHICAL.

A Study of Ethical Principles. JAMES SETH. New York: Imported by Charles Scribner's Sons. 1894. Pp. XVI, 460.

The subject is presented in three parts;—(I) an analysis of the psychological basis of ethical principles, and criticism of the corresponding systems; (II) a discussion of the virtues, under the caption of The Moral Life; and (III) the metaphysical implications of ethics. In Part I, Prof. Seth criticises Hedonism as unduly emphasizing the sentient nature of man on the one hand, and so-called Rigorism on the other as laying exclusive stress upon man's rational nature. Each is based upon a partial psychology, and hence incomplete and misleading. He would therefore distribute the emphasis, so that the total personality embracing both sensibility and reason is regarded as the proper basis of ethical principles. This personality differs from the lower, or animal self-hood of mere individuality in the power of transcending the entire impulsive and sentient life, subduing it unto the higher rational self. This power constitutes the will, and differentiates man from the animal. Following the epistemological analogy, as the Ego constructs the various data of sensation through the apperceptive process, forming out of them an object of knowledge, so in the construction of the moral end out of the impulses, there is a similar synthesis of the crude data of sensibility. Prof. Seth's ideal, therefore, is self-realization, and his ethical system he styles Eudaimonism, wishing to restore its original Aristotelian significance which presented pleasure as 'the very bloom and crown of goodness.' We question the propriety of using

a word so strongly associated with Hedonism, to characterize a system which subordinates the pleasure elements to a superior ideal. It seems more appropriate to classify Prof. Seth among the following of Hegel and Green, on account of the prominence of the idea of self-realization in his scheme. He, however, contends that they have under-estimated the sharpness of the existing dualism, in affirming the essential rationality of the life of sensibility; and that the full force of the antithesis must be appreciated in order to realize that complete synthesis where inclination and duty are one.

Prof. Seth criticises Rigorism as presenting an abstract formal law of conduct, which, however, is devoid of any definite content. But does he himself escape a like imputation?—inasmuch, as since his ethical ideal is the realization of self-hood, or personality, the question naturally arises, what self? What kind of personality? It is the self constituted by the common rational Ego in each individual. But such an ideal is formal, and as truly lacks a definite content, so that we are confronted by the old difficulty in a new form. In Part III, the metaphysical implications of ethics, the author treats of freedom, God and immortality as necessitated by his doctrine of personality. Since self is more than the sum total of sensations, it is so far forth superior to the sensuous stream of consciousness, and therefore free. Also self seeks a larger environment than Nature, that is God; whence immortality is naturally deduced. This also furnishes a supplement to the Kantian theory of Autonomy, inasmuch "as the moral law is the echo within our souls of the voice of the Eternal, whose offspring we are."

The Kantian Epistemology and Theism. C. WISTAR HODGE. Philadelphia, MacCalla & Co., 1894. Pp. 47.

The author criticises Kant's epistemological position, because he does not follow it out to its logical conclusion. Kant's primary presupposition that all things exist only in relation to self-consciousness necessitates a second presupposition deducible from it, namely, that the real is the rational. This is overlooked; and the spirit of Kant's teaching as well as the logic demands its recognition. Dr. Hodge contends that the ideas of reason must be more than mere logical universals; that there is a necessary and vital connection between knowledge and being within our consciousness, that the activity of the mind necessitates its universality and reality; that there is a unity of organic experience, and an objectivity of the categories. By an acute analysis of Kant's position, he seeks to prove the necessity of these supplementary propositions, especially as throwing light

upon Kant's theistic criticism. He shows that there is need to emphasize God's immanence as well as His transcendence. The two ideas are not mutually exclusive but are realized in one self-conscious and personal spirit; and Kant's criticism of theistic arguments proceeds upon the basis of a transcendent Being merely, the mechanically conceived God of Deism. Finality also must be conceived as an objective fact; and this strengthens the reasonableness of the theistic position. Moreover the Kantian ethics in relation to theism, overlooks the fact that the self-revealing spirit can be like us because we are formed in His image; and if our noumenal self carry with it a moral ideal, so must God also be conceived as possessed of moral attributes.

The Æsthetic Element in Morality and its Place in a Utilitarian Theory of Morals. FRANK CHAPMAN SHARP. New York, Macmillan & Co., 1893. Pp. 131.

The problem presented in this work is to determine the place of beauty of character in the moral world. Dr. Sharp approaches this problem from two different sides. First as to its origin, he contends that every attempt to make beauty of character the primary product of the moral forces is doomed to failure, and he insists that their original goal is the general happiness according to a utilitarian criterion of right and wrong. In the elaboration of this, he shows affinity of thought with Shaftsbury and Hutcheson. From a second point of view, he discusses the question of values, and reaches the conclusion that beauty of character is but one of the many sources of æsthetic emotion, and that its attraction for us is due to the pleasure it affords, the worth of which is to be measured by the same scale which we apply to the other emotions. In all this, the author seems to us to be himself alive to his omission of the idea of obligation as an essential factor. For he supplements his discussion by an analysis of the idea of oughtness. This is for him in the main, the pressure of the accumulated judgments of society upon the individual consciousness. This overlooks the principle of autonomy; and man's will then feels obligation only as imposed from without. This is especially unsatisfactory where Dr. Sharp speaks of the 'theological ought.' He regards God in the same manner as society, or our fellow men in general, one of the powers without, which expect from us certain lines of conduct. Such a view makes obligation unreal, by presenting it as a force both arbitrary and artificial.

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NEUROLOGY.

Ueber die Histogenese der Körner der Kleinhirnrinde. ERNST LUGARO. Anatomische Anzeiger, No. 23, 1894.

Recent studies in the development of the nervous system have brought to light some interesting and curious migrations of nerve cells. His showed that the neuroblasts arising from germinal cells about the central canal must migrate to the different parts of gray matter in which they underwent their further development. The migration of a spherical cell probably possessing amœboid powers is familiar enough from the manner in which the leucocytes travel through the fixed tissues. But in some way nerve elements, if the interpretations given are correct, must traverse the tissues even after their prolongations have been formed.

The paper in question is a research on the granules or small cells of the cerebellum. On the surface of the developing cerebellum is a layer of epithelium like cells with one process passing directly towards the surface and the other more or less in the opposite direction. These elements shade off by intermediate forms into dineuric cells, the two neurons of which run parallel with the cortical surface. Among those lying somewhat deeper, appear dineuric elements from which a dendritic process has begun to grow away from the surface. Just as in the case of the dineuric cells of the spinal ganglia, the neurons come to lie more and more at one side of the cell body ; finally a stem is formed and this stem lengthens, the cell bodies sinking deeper into the granular layer. The dendritic processes appear first as a single conical outgrowth, then become branched, multiple and shorter, and finally decrease in number with the formation of the characteristic brushes at their termini and become identical with the cells of the fully developed granular layer, the very long stem of the neuron passing into the molecular layer and terminating in the T process now so well known from the studies of Cajal, Köl-liker and others.

If this is a true history of the development of these granules, the author is justified in his conclusions: (1) The position of the nerve cells durlng embryonic life is not necessarily that which it will finally hold. (2) The neuron grows not only at its ends but also throughout its entire length, thus permitting the cell body to wander. (3) While the neuron undergoes a gradual and progressive development, the dendritic processes may at some intervening time be better developed than they are at the end of growth.

The disappearance of the primitive and intermediate forms as the

final form becomes more abundant, is one of the arguments in favor of the author's view; but there is no escaping the conclusion that not only the cell-body with its dendrons must then move through the surrounding substance, but the neuron with its termination also moves. While therefore the argument in favor of these changes appears complete, it requires us to admit that the nerve cell with all its prolongations may, during growth, sink through the substance of the cerebellum, like a bullet through a plate of wax.

Although these experiments do not stand alone, nevertheless so difficult is it to accept this explanation that careful control observations on displacements due to the enlargement of other nerve structures should be made before entertaining too seriously the conclusions to which these results apparently point.

Beitrag zur Kentniss der Pathologischen Anatomie der Paralysis agitans und deren Beziehungen zu gewissen Nervenkrankheiten des Greisenalters. Emil Redlich. Jahrbücher f. Psychiatrie, Bd. XII. Heft 3.

In connection with the changes occurring in the nerve cells in old age, the observations on the nervous system in paralysis agitans are of importance. The paper by Ketscher, 1892, on this subject, indicated that the appearances found in the nervous system of the aged suffering from paralysis agitans were in kind similar to those found in persons who, though aged, did not exhibit this disease. The difference between the appearances in the two sets of cases were in his opinion mainly one of degree.

In view of Ketscher's publication, Redlich gives his results in a condensed form, as they are mainly confirmatory, though in part divergent. In paralysis agitans the spinal cord is especially affected, and in it the lumbar and cervical enlargements are the centres of greatest change. Here the most marked alterations are in the lateral and dorsal columns where there is a sclerosis with atrophy of the nerve fibres, vascular changes with the formation of amyloid bodies, while the cells in the ventral horns and the columns of Clarke are often so pigmented as to obscure the nucleus. In other portions of the central and peripheral system similar changes occur, though they are less intense.

Redlich urges a sharper distinction between the condition of the central nervous system in uncomplicated old age and those found in paralysis agitans. He would separate the pathological anatomy in these latter cases from those of simple senility by the greater changes in the blood vessels and formation of sclerotic areas due to the in-

crease in the supporting tissues, changes which do not necessarily accompany the involutionary process in the central system.

Histological changes induced in sympathetic, motor, and sensory nerve-cells by functional activity. (Preliminary note). GUSTAV MANN. Jour. of Anat. and Physiol., N. S., Vol. IX., Part I., October, 1894.

Previous to this publication, there have been current two statements concerning the effect of electrical stimuli on the size of nerve-cells and their nuclei. Hodge, working with weak faradic currents, applied intermittently, but for a long time, to the sensory spinal nerves of the frog and cat, found the cells of the spinal ganglia thus fatigued, to be shrunken. Vas, with stronger stimuli applied for fifteen minutes to the trunk of the cervical sympathetic of the rabbit, found the bodies and the nuclei of the sympathetic cells thereby swollen.

To the examination of this discrepancy the author has directed his observations. He repeated Vas' experiments under his conditions, and obtained similar results; a swelling of the cell-body, its nucleus, and the structures within the nucleus. On examining parts of the nervous system of a dog after prolonged muscular exercise, the motor cells in the lumbar region of the cord were found to exhibit the characteristic shrinkage described by Hodge, while cells from the motor region of the cortex showed swollen bodies and nuclei.

When the two retinæ of a dog were compared, one retina having been at rest while the other had been stimulated, both results were obtained within the limits of the same retina; the nuclei of the rods showing decided shrinkage while those of the middle ganglion-cell layer were swollen.

These differences lead the author to suggest that *activity* in nerve cells is accompanied by an increase in volume, whereas *fatigue* is associated with the reverse change. These terms are not happily chosen, since both apply equally well to the entire series of changes taking place in the cell. With the commencement of activity, fatigue commences and they continue together as different aspects of a single process. It would therefore be better to distinguish between the earlier and later phases of one or the other. In the early stages of fatigue, associated with enlargement, M. finds that the cell, and especially the nucleus, become chromophobic, and it is interesting to note that in his study of the mature cells, in the cervical enlargement of the mammalian cord, Kaiser found the chromophobic cells to have the greater mean diameter.

Like all such matters the histological changes accompanying the functional activity of nerve cells, becomes more complicated, the further it is examined, and it is to be hoped, that it will in this instance be possible to unravel these apparently opposite reactions without any obfuscation of the main point, due to the neglect of the mechanical conditions of experiment, or of the full record concerning the species, age, sex and physiological condition of the animals employed.

Quelques observations expérimentales sur l'influence de l'insomnie absolute. MARIE DE MANACEÏNE. (Address made before the International Medical Congress held at Rome, 1894). Archives italiennes de Biologie, 1894. T. XXI.

Dogs from 2 to 4 months of age were prevented from sleeping and the physiological and anatomical effects of this treatment, recorded. It appears that, for these animals at least, loss of sleep is much more detrimental than starvation. Dogs after starving more than twenty days and having lost more than 50 per cent. of their initial weight, may still recuperate under favorable conditions; but loss of sleep for four or five days is fatal. The temperature of the sleepless animal finally falls as much as 8° below the normal, the reflexes disappear, the red blood corpuscles first diminish in number, to undergo a final increase during the last two days, when the animal refuses food.

Fatty degeneration of the tissues was the chief histological change noted at the post-mortem examination; the blood vessels often appeared compressed, were surrounded by leucocytes and there were capillary hemorrhages on the surface of the cerebral hemispheres, with more extensive ones along the optic pathway; while the spinal cord appeared abnormally dry and anæmic.

When it is remembered that the central system withstands the effects of starvation in a most remarkable manner, maintaining almost its full weight up to the death of the animal, the great disturbance following a few days' loss of sleep is very impressive.

Zur Kenntniss der Veränderungen des Rückenmarkes beim Menschen nach Extremitätenamputationen. A. GRIGORIEW. Zeitschr. f. Heilk. 1894. Bd. XV.

The physiological conditions on which a mature nerve cell depends for its healthy maintenance are complicated. Most important are the supply of nutritive substance; the regular alternation of ac-

tivity with repose and anatomical completeness; but the essential feature in each one of these conditions is far from clear.

G. has examined the spinal cord in the five cases of amputation, two at the upper arm, two at the thigh, one below the knee. The last lesion was but a year old when examined, and the cord showed no changes. In the other four cases there was an atrophy on the corresponding side of the cord, noticeable earliest in the dorsal roots and dorsal columns, later in the cells of the ventral horns. The degree of the atrophy increased with the age of the lesion and was accompanied by some degeneration. These results are mainly confirmatory of previous observations. On attempting to analyze them it will be readily seen how complex they are.

In these cases both the sensory and motor elements have lost portions of their neurons without an opportunity to regenerate them. The sensory elements have been deprived of the major number of stimuli coming to them under normal conditions, and have thus failed to send the usual impulses on to the cord. The motor elements have been crippled by the loss of their peripheral outgrowths and allowed to become torpid by disuse. There are, to be sure, other impulses coming to these cells, in addition to those arriving by their associated dorsal roots, and they must, for some time at least, retain the power to discharge along their efferent prolongations. To explain this atrophy in the cord after amputation, it will therefore be necessary to learn what atrophic changes occur, when either root, a dorsal or ventral, alone is cut and for what period after operation the cell bodies continue to send out impulses.

Note on the Degenerations following Double Transverse, Longitudinal, and Anterior Cornual Lesions of the Spinal Cord. ALBERT S. GRÜNBAUM. Journ. of Physiology, Vol. XVI.

On isolating by double transverse section about three spinal segments in the lower thoracic region of the cord of a monkey, it was found that there followed a very complete degeneration of the dorsal columns, and in the remaining columns a sharply defined peripheral band marking out the distribution of the long tracts at the circumference. The degeneration increased with time, being greater at the end of the six months than at the end of one. After this lesion the included anterior root fibres do not show degeneration, indicating that they rise within the limits of the portion isolated, but when the ventral cornu of one side has been destroyed, some degenerated fibres are to be found in the opposite ventral root (observations on cats). A longitudinal mesal section of the cord was made (also on

cats) with the result of an ascending degeneration just external to the dorsal roots, and another at the ventral border of the cerebellar tract. This latter is explained as due to the section of fibres crossing in the anterior commissure, while the former is attributed to injury of the columns of Clarke.

Report of the Versammlung deutscher Naturforscher und Aerzte in Wien vom 24–30 September, 1894; *Section für Psychiatrie und Neurologie.* Neurologisches Centralblatt, No. 20. 1894.

From the reports of a number of papers of neurological interest, given at the Vienna meeting of the German Society of Naturalists and Physicians, the following abstracts are taken:

Ueber die topographischen Beziehungen zwischen Retina, Opticus, und gekreutzen Tractus beim Kaninschen. A. Pick.

From the destruction of limited areas in the retina, and the study of the subsequent degeneration, it appears that the affected fibres occupy in the cross section of the opticus or tractus a position corresponding to that of the injury in the retina. No allusion is made to facts bearing on the bundle of uncrossed optic fibres, though there is reason to think that in the rabbit this is connected with the lateral portions of the retina.

Die Sinnesorgane und die Ganglien bei Anencephalie und Amyelie. O. v. Leonowa.

In a fœtus of eight months in which the medullary tube was lacking, the author had previously shown the presence and moderate development of the sensory system. In this new case, also a fœtus of eight months, anencephalic and amyelic, the nervous system consisted of the retina and of spinal and sympathetic ganglia, with the nerves arising from them. A portion of these nerves could be followed into the muscles. The spinal ganglia were not completely separated from one another, but the dorsal roots had been formed and, except in the cervical region, these roots ran cephalad. The nerve cells were complete; some of the fibres medullated, but in the so-called retina there were no nerve cells, neither were there any fibres in the optic stalk. The independent development of the sensory system, the sympathetic being associated with it, the distribution of sensory nerves to the muscles, and the normal development of the musculatur, are the striking features of this case. The last fact is very interesting, for in normal persons the severance of the ventral roots from the muscles leads to degenerative changes in

the latter, and the fact that the muscles can develop without such a nerve supply is most remarkable.

Ueber die feinere Anatomie und die physiologische Bedeutung des sympathischen Nervensystem. v. KÖLLIKER.

The medullation of sympathetic fibres is very irregular, and they exhibit every combination which can occur. K. considers the functions of the sympathetic system partly independent—an expression not explained—and partly dependent on the central system. The cells he describes as mononeuric. Those cells with a spiral and straight fibre give origin only to the latter, while the former originates elsewhere and merely terminates on the body of the cell. In the mammals these sympathetic cells may be multipolar. In this latter case, the dendrons are pathways for afferent, and the neuron, for the efferent impulse.

In this view the cells are mainly efferent or motor, while the afferent impulses, passing first to the medullary centres, are mediated by a few fibres from the dorsal spinal roots. It is not easy to see how independence can in any sense be granted to this system, unless the anatomical arrangements for both afferent and efferent impulses are present in it. It is suggestive to recall, moreover, that the most probable point of origin for the sympathetic cells is the same as that which gives rise to the cells of the spinal ganglia, which are the typical sensory elements. H. H. D.

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VISION:

OSCILLATIONS IN THE RETINAL PROCESS.

(*1*) *Ueber die nach kurzdauernder Reizung des Sehorgans auftretenden Nachbilder.* CARL HESS. Pflüger's Archiv. XLIV, 190. 1891.

(*2*) *Ueber Nachbilder.* SNELLEN. XXIII. Vers. ophth. Gesellsch. zu Heidelberg, 1893.

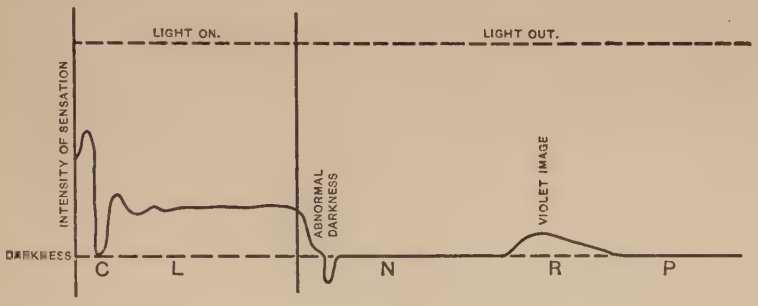
(*3*) *Primäre, secundäre und tertiäre Netzhautbilder nach momentanen Lichteindrücken.* H. P. BOSCHA. Arch. f. Ophth. XL, (1), 22–42, 1894.

(*4*) *Studien über Nachbilder.* CARL HESS. Arch f. Ophth. XL, (2), 259–279, 1894.

(*5*) *On the Recurrent Images following Visual Impressions.* SHELFORD BIDWELL, Proc. Roy. Soc. LVI., June 7, 1894.

Of these several papers on after-images, the last is the most important. The experiments of Mr. Bidwell (like those of Hess in his

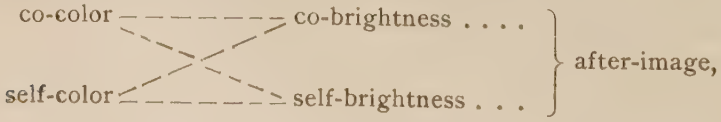
second communication) concern the phenomena which arise when bright colorless or colored objects are made to move rather rapidly before the eye in an otherwise dark room. Hess used small glow lamps of .5 cm diameter at a distance of about half a meter from the eye, covered with glass of different colors. Bidwell found colored glasses quite inadequate to his purpose, and hence made use of homogeneous light, obtained by a high pressure oxyhydrogen light, a bisulphide of carbon prism, a screen with a slit in it, and a mirror which rotated about a non-perpendicular axis and so caused the portions of the spectrum reflected from it to describe a circle upon a second screen. With various modifications of this apparatus he obtained a complicated series of sensations which will be best borne in mind if we reproduce his diagram :



Immediately upon the impact of the light there is experienced a sensation of luminosity, the intensity of which increases for about one-sixtieth of a second. Then follows suddenly a sensation of darkness, lasting also for about one-sixtieth of a second ; this is the now well-known Charpentier oscillation. Several slight waves follow this, and then there is a period of steady luminosity, which, however, is much less intense than the first instantaneous maximum. Upon shutting off the external light, a sensation of diminishing brightness continues for a brief interval, and is followed by a "sudden and clearly-defined sensation of what may be called abnormal darkness—darker than common darkness—which lasts for about one-sixtieth of a second," and is followed by another interval of ordinary darkness (N). Finally there occurs another transient impression of luminosity, *generally violet-colored* (R). This is the Recurrent Image, which is the special subject of this paper. The recurrent image is followed by what the author speaks of sometimes as a period of steady darkness—" after which the uniformity of the darkness remains undisturbed" (p. 143)—but at other times he re-

fers to it as a phosphorescent trail, the color of which cannot be determined, but which is with some observers "so intense that the recurrent image cannot be distinguished from it at all." This whole period (R and P) is what observers in general put under the head of the positive after-image; it has almost always been supposed to be continuous with the original sensation, and its color (which according to all observers is most persistently a reddish violet) is what Helmholtz describes as *farbiges Abklingen des positiven Nachbildes*, a term which will no longer be appropriate. The fact that the positive after-image is preceded by a negative phase (N) has recently been made plain by Hess (1), who points out that it has been wholly overlooked by Helmholtz, Aubert and Fick; it had, however, been distinctly described by Purkinje, whose works are still a store-house of facts which await a sufficiently careful observer to be rediscovered. Bidwell, strangely enough, wrote in ignorance of this paper of Hess, and Hess was also unaware that the interval of darkness which interrupts the positive image had already been studied by Prof. C. A. Young and by Mr. A. S. Davis (Phil. Mag., Vols. 43 and 44, 1872). The fact that when the after-image of the whole spectrum is formed, its brightest part is not in the yellow but in the green (Hess and Bidwell) will doubtless be found to connect itself with the fact that the green is the brightest part of the spectrum when seen in a faint light.

All recent work in vision shows that it becomes more and more necessary to distinguish between the specific sensation (color) and the absolute sensation (brightness) produced by objective light, and in particular the terms positive and negative as applied to after-images are very misleading unless farther particularized, for an image may be positive in one respect at the same time that it is negative in the other. It is, for instance, on account of this ambiguity, an almost hopeless task to endeavor to compare the phases of the after-image as described in the five papers whose titles are given above. In order to secure comprehension and at the same time to avoid the use of such complicated phrases as "an after-image which is complementary to the original one in color but of corresponding brightness," I would propose, in the absence of a less awkward terminology, the following; *co-color*, as an abbreviation for complementary color; *self-color* for a revival of the same color, and the same prefixes for the phases of brightness. Thus we should have:



for the four possible descriptions of after-image, and for the above long phrase we should substitute the *co-color self-brightness image.*

Boscha (3), following out the preliminary work of Snellen and in his laboratory, used an electric spark in a dark room to illuminate colored papers. His secondary stage is one of co-color and self-brightness, and it is followed, after a pause, by an image which is always of a peculiar reddish color difficult to define. He makes a number of criticisms upon the work of Hess in (1), which are shown in (4) to be based upon misconceptions. The dark interval preceding his secondary stage he seems to have quite overlooked. The conclusion of the whole matter is that there are numerous oscillations in the chemical process which is set up in the retina by light, whose farther more detailed study may be expected, perhaps, to throw additional light upon rival theories.

COLOR BLINDNESS.

(1) *Physiologische Analyse eines ungewöhnlichen Falles partieller Farbenblindheit.* II. M. v. VINTSCHGAU. Pflüger's Archiv, LVII. 191–307.

(2) *Ueber einen Fall von Gelb-Blaublindheit.* EW. HERING. Pfl. Arch. LVII. 308-332.

Prof. Langley has lately been so fortunate as to be able to perfect his methods for the determination of the cold lines in the infrared part of the spectrum to such an extent that work which it formerly took him a year to finish he can now do in a fraction of a day, and he has already laid down 2,000 lines with far greater accuracy than he could otherwise have obtained them with at the end of a hundred years. A somewhat similar change has been effected in our power to determine the exact nature of a given case of color blindness by Prof. König's latest form of the Helmholtz color-mixing apparatus, or spectro-photometer for colors (an improvement even over the instrument exhibited at the Chicago Fair), when manipulated with all the precautions and the corrections which are made use of in König's laboratory. If v. Vintschgau had been willing to make use of this method, he might have described his case of dichromasie not in one hundred and sixteen pages, but in three, one of which would have contained two diagrams, one giving the patient's brightness curve through the spectrum, and the other giving the curves for his two color sensations, as deduced by the relative amount of the two end-sensations necessary to match in tone and brightness all the intermediate parts of the spectrum. This

simple information we seek in vain in v. Vintschgau's pages, but we have instead endless series of experiments with colored paper, glass, and wools, which all belong to a previous period of color-blindness investigation, and which simply repeat the result obtained by his spectrophotometer,—namely, that the patient lacks the sensations blue and yellow, as in a typical case of blue-blindness, and that he has a slightly diminished sensibility for the colors that remain to him, for the short-wave end of the spectrum rather more than for the long-wave end. This is hardly enough to make the case 'unusual,' and Hering refers to it simply as *a case;* the adjective in v. Vintschgau's title is, in fact, simply a survival from his first communication, when he had convinced himself, also by an immense number of experiments, that his patient could see yellow, which would indeed have been an anomaly. This mistake he was led into by paying attention to the *names* which his patient used for his sensations. How wholly unjustifiable this is was pointed out by Dr. William Pole, himself partially color-blind, in an article of most remarkable logical acumen, in which the writer wholly anticipated the facts in regard to color-blindness which have only with the last few years gained acceptance (*Trans. Roy. Soc.*, 1858). The violet end of the spectrum gives this individual no color-sensation when looked at directly (2), but it has been possible to prove indirectly, and in three different ways, that he has here a sensation of red; (*a*) a little of this light when added to a red which is too faint to be distinguished causes a plain sensation of red; (*b*) it forms a colorless mixture with green, and (*c*) it gives a distinct green as a contrast-color. This fact that a color too faint to be perceived may yet cause its proper contrast effect to be very distinct is already known, and is beautifully exhibited in the experiments with a projection lantern of Rollet (Pfl. Arch. XL. 25, 1892,) the most brilliant yet made in the subject of contrast.

The necessity of paying no attention to what the patient says that he sees, but only to his color equations, is now pretty well recognized, and Hering himself (but not v. Vintschgau, p. 205) distinctly insists upon it. Nevertheless, in calling the two color-sensations of the person here investigated red and green, Hering (and v. Vintschgau as well) overlooks this precaution, and at the same time assumes the correctness of his own theory, which the case is supposed to support. All that we know is that this patient has some distinguishable color-sensation along the spectrum as far as λ 596, and again another sensation from λ 574 to λ 481. *If Hering's theory is correct*, this latter sensation is green; upon the Helmholtz theory, as

held by its present defenders (and upon any three-color theory), it is blue-green; the whole bearing of the case upon existing theories depends upon whether this color (the complement to the fundamental red of both theories) is properly named green or blue-green; in other words, it has no bearing at all upon them. König has very recently had several most interesting cases of monocular blue-blindness circumscribed within a small area surrounding the fovea, in which the sensation given by the cold end of the spectrum can be readily compared with normal sensations, and is found to be blue-green. This fact is still, of course, not decisive between theories, for what Hering means by green is a color which is pronounced to be blue-green by the ordinary consciousness, and this in spite of the fact that it is precisely for the satisfaction of the ordinary consciousness as regards color-sensation that Hering's theory has been devised.

C. Ladd Franklin.

SENSATIONS OF TASTE.

Contributions to the Physiological Psychology of the Sense of Taste. Friedrich Kiesow. Abstract of papers in Philosophische Studien, Bd. X, Heft 3, pp. 329 ff; Heft 4, pp. 523 ff.

The electric, metallic and alkaline tastes being reserved for special investigations, the present work treats only of those sensations recognised as special qualities, viz., sweet, sour, salt and bitter. The taste-substances used were: chlornatr., muriatic acid, sacch. alb., sacch., quin. sulph., and quin. pur. In all cases in which chemical combination of the substances was to be strictly avoided quin. sulph. and not quin. pur. was used. The greatest possible chemical purity was sought for these substances, which were dissolved in distilled water. The application was made partly by means of dropping-glass tubes on which a scale graduated by $\frac{1}{10}$ cm. was engraved, partly by means of soft pointed hair brushes. All disturbing accompanying sensations, not excepting that of temperature, were excluded. The simplest way to accomplish the last was to raise the fluids to be applied to the temperature of the mouth, viz. 37° C. Between the separate experiments the mouth was rinsed out with pure water of the same temperature, 37° C. After having trained the subjects, I first examined the cavity of the mouth, with a view to determining what parts were receptive of sensations of taste. These experiments were performed both on children and on adults. Taking into consideration what former investigators have found out the total results of this chapter will be given.

1. Besides the whole surface of the tongue together with its base and the under surface of its tip—the hard and soft palate, without doubt the arcus glosso-palatinus, the tonsils, the uvula, the isthmus fancium, the inside of the epiglottis and the mucous membrane of the cheeks participate in the sensation of taste.

2. All these parts are sensitive in childhood; in adults the mucous membrane of the cheeks, the middle of the tongue and, with a few exceptions, the hard palate lose their sensitiveness. In some cases the under surface of the tip of the tongue on both sides of the frenulum remains receptive also in adults.

3. The presence of disturbance is accounted for, sometimes by an affection of the cavity of the tympanum, sometimes by individual differences.

It must be remarked that the perceptive faculty of the inner epiglottis was established by Michelson and Langendorff,[1] that of the mucous membrane of the cheeks in childhood by Urbantschitsch.[2] Concerning the retrogression of certain taste surfaces in adults I must refer the reader to my longer article in which an explanation according to the theory of development is offered and literary references given.

In a further investigation I tested the sensitiveness of the different perceptive parts of the cavity of the mouth, by taking as measure the absolute given for the different qualities of taste, obtaining in this way the following general results:

1. Sensitiveness varies for the different qualities on the different parts of the tongue. Sweet is tasted best on the tip of the tongue, sour on the edge, and bitter at the base, acid equally on the tip and edges, but less at the base.

2. With regard to the values found in an isolated case for the other taste-surfaces, the sensitiveness for sweet and bitter appears in the following order: Soft palate, arcus glosso-palatin., uvula, under-surface of the tip; for sour, arcus glosso-palat., palat. molle, uvula, under-surface of the tip; for salt, palatum molle, under-surface of the tip, arcus glosso-pal., uvula. The values are in part considerably below those noted under 1. Only on the soft palate does salt reach the normal given.

3. A single investigation showed that in childhood all parts, excepting the tip and edges of the tongue, possessed nearly the same

[1] Centralblatt für Physiol. 1892. P. 204.

[2] Urbantschitsch, *Beobachtungen über Anomalien des Geschmacks, etc., in Forge von Erkrankungen der Taukenhöhle.* 1876.—I desire to draw special attention to this interesting work.

sensitiveness with regard to sweet. The tip and edges were more sensitive.

4. The explanation of the normal condition, as of individual differences is without doubt to be found in the law of adaptation, excepting those cases in which pathological causes, obstructions, etc., appear.

Further, attention was directed to the qualitative conditions of the sensations of taste. These experiments were only made on adults. First, I was enabled to prove that all four above-named qualities are true sensations of taste, also that the sensations of sour and salt must not be excluded from the sphere of taste on account of the accompanying tactile sensations. On the contrary, my investigations led to the conclusion that all our perceptions of taste are accompanied by tactile sensations, although in different degrees. Sweet is accompanied on and near the limen by a sensation of smoothness, at higher intensity by that of slipperiness, at very great intensity by that of scratching and biting. The liminal values of bitter are accompanied by a distinct sensation of greasiness. Even the application of distilled water produced with some of my subjects a distinct perception of taste. Two of them tasted water on the tip of the tongue as sweet, on the edges as sour and sourish, at the base bitter. Others tasted it as bitter in the whole cavity of the mouth, others only bitter at the base and tasteless on the other parts of the tongue. The bitter sensation produced by distilled water accompanied the single sensation called forth by taste—substances often for a time above the limen, so that in this way two sensations arose which I have designated as double-sensations. Even a mechanical stimulus of the base of the tongue with a glass-rod produced with me and with many of my subjects a sensation distinctly bitter.

Great influence in the region of taste must be ascribed to association and the effects of contrast. The conditions of contrast I investigated with special care; the total results of which may be given concisely as follows:

1. Contrasting stimuli must be recognized in the sense of taste.

2. Salt contrasts with sweet, salt with sour, sweet with sour.

3. Salt and sweet, and salt and sour contrast both on simultaneous stimulation of corresponding parts of the tongue and on successive stimulation of the same taste-surface. The contrasts of sweet and sour could only be observed in the latter case.

4. Bitter forms an exception, but yet perhaps gives rise to contrasts restricted to individuals. THE AUTHOR.

LEIPZIG.

EXPERIMENTAL.

Psychologie des grands calculateurs et joueurs d' échecs. A. BINET. Paris, Hachette. 1894. Pp. VIII, 364.

Travaux du Laboratoire de psychologie physiologique de hautes-études à la Sorbonne. BEAUNIS, BINET and others. Année, 1892; pp. 100; année, 1893; pp. 58. Paris, Alcan. 1893–4.

Travaux du Laboratoire de psychologie physiologique pendant l'année 1892–1893. A. BINET and others. Rev. Philos. XXXVII, 111–119, 222–240 and 344–352. 1894.

The first two numbers of M. Beaunis' Annual, (*Travaux du Laboratoire,*) indicate the main lines of research that have been followed thus far in the new psychological laboratory at Paris, of which M. Binet is the leading spirit. Several of the studies appear also, as preliminary reports, in the pages of the *Revue Philosophique.* The investigations can be grouped for the most part under two general heads, *audition colorée* and memory; the latter are incorporated in M. Binet's work on *Great Calculators and Chess Players.*

M. Binet was able to secure two noted calculators and test their powers and methods at some length. One of them, M. Inaudi, was distinctly of the auditory type, preferring to receive his data orally; when required to start with written numbers, he always repeated them to himself, before proceeding with the problem. The other, M. Diamandi, a Greek, was quite as markedly of the visual type. M. Binet devotes a chapter of his work to some tests made on these two, together with a distinguished prestidigitator, M. Arnould, to determine their comparative facility in memorizing and repeating numbers. M. Arnould's method is to associate each numeral with a certain consonant; by adding vowels at will, he transforms any given number into some word or phrase, which he readily memorizes; when called on he simply translates this back into the number. M. Inaudi could memorize the most rapidly of the three up to a hundred figures, (36 in 1m 30″, 75 in 5m 30″, 100 in 12m.), which was as far as he was tested. As between MM. Diamandi and Arnould, the former learned the first few figures somewhat more quickly, but from 25 places on the latter had a decided advantage; (D: 10 in 17″, 25 in 3m, 50 in 7m, 100 in 25m; A: 10 in 20″, 25 in 2m 30″, 50 in 2m. 45″, 100 in 15m). The two 'direct' memorizers were compensated, however, by being able to *repeat* what they had learned much more rapidly than M. Arnould. The investigation is particularly interesting in view of the dispute as to the

value of mnemonics. It would be interesting to test also the comparative *durability* of the several kinds of memory by experiments similar to those of Ebbinghaus. M. Inaudi was able to repeat 230 numerals that he had learned on the preceding day, but the experiment was not followed out; it seems probable that M. Arnould would have been able to retain many times that number. (Mnemonic memorizing, which relies on mediating associations, might be called the *associative* type, to distinguish it from the auditive and visual, where the correlation is immediate.) — M. Binet endeavors to account for the rapidity with which MM. Inaudi and Diamandi perform complicated mathematical operations, in a number of ways. (1) By the use of various devices which simplify the problem. (2) By an extension of the multiplication table to products of two place numbers. (3) By the aid of unconscious mental processes: just as we unconsciously adopt results in multiplication, from the memorized table, so one accustomed to calculating might come upon results intuitively, which he would require a long and tedious operation to reach, if every step had to be formulated. Proceeding mechanically or half-consciously, he would not need to use words or symbols to indicate the operations; this abbreviation would in itself shorten the time considerably, and M. Binet agrees with Scripture in emphasizing its importance in the 'shorthand' of calculation, even if the process be wholly conscious and voluntary.

The second part of M. Binet's work treats of blind-fold chess-playing. By means of a *questionnaire* and personal letters, he obtained from a large number of players more or less complete analyses of their methods of procedure. According to this testimony, there are three principal requisites for success: (1) Familiarity with the game, through practice and study; especially a knowledge of the best line of play for various positions, in order to save time and thought. This is far more essential here than in play over the board. (2) Memory, in order to retain the moves already played or the position of the pieces at a given time. (3) Power of visualization. This last is the distinguishing feature of blind-fold playing. In some players it takes a very concrete form: they seem to *see* the squares, dark and light alternately, and the pieces upon them more or less distinctly. In other cases it is rather more symbolic—*abstract*, as M. Binet terms it; the player sees only part of the board at a time, and represents in a vague way the pieces and their positions; but he works by means of the *potency* of the pieces,—an idea, rational rather than visual, of what each one can do from its given position. In addition to this there is in some cases a *verbal* memory; *i. e.*, an auditory memory of

the names of the squares and moves; a great many players repeat the moves in a low tone of voice, to re-enforce the visual memory-image.

A different phase of memory from those treated in this book, is taken up in a study of the visual memory of children; 225 school-children, from 9 to 13 years of age, being examined in groups of four. They were shown a line, and afterwards asked to reproduce it, or pick out one of similar length from a set. Three standard lengths, 16, 40, and 68 mm., were used. M. Binet does not mention the lapse of time between the original presentation and the choice or attempted reproduction (10 to 15 min. ?), nor the differences in length of the lines given to choose from in the second place. The absence of these data prevents us from comparing his results with those of a somewhat similar investigation recently conducted at Princeton, (using squares, however, instead of lines), which will be reported in a succeeding number of this REVIEW.

Among the laboratory studies are several on *audition colorée*. M. Binet and his colleagues examined a number of persons subject to this phenomenon, in order to determine the rapidity with which they associate the color with the word or vowel. So far as they go, the figures seem to show that this peculiar color association is not more rapid than other kinds of association; M. Binet shows by experiments on himself, that one who has not colored audition may *learn* a set of associations between vowels and colors so thoroughly, as to be able to call up the proper one in each case, and may do so even more rapidly than those who make the association naturally.

PRINCETON. H. C. WARREN.

RECOGNITION AND ASSOCIATION.

Assimilation and Association. (I and II). JAMES WARD. Mind, N. S., II, p. 347, July, 1893, and III, p. 509, October, 1894.

By assimilation, Dr. Ward means the recognition, or as he prefers to say, the cognition, involved in perception, that is, what Höffding names *unmittelbares Wiedererkennen*. His central position is the assertion that assimilation, in this sense, so far from being identical with association or explained by it, is really the necessary presupposition of association, which is obviously between *recognized* objects of consciousness. The most important part of the discussion is the vigorous criticism of the view that assimilation is really a case of 'association by contiguity' and that, accordingly, an object seems familiar through the presence of faint representations, *Nebenvorstel-*

lungen of events or objects, formerly connected with the familiar one. Dr. Ward's objections to this theory are based largely on physiological considerations. From Müller, Siemerling and others, he quotes cases of cerebral disturbance, in which "visual memory images are for the most part retained, so that old scenes can be recalled and familiar objects or persons accurately described, and yet the recognition of them is no longer possible, when such persons or objects are actually present." Dr. Ward insists, perhaps without sufficiently considering the possibility that apparent concrete visual memory-images may really be verbal images, that such cases would be impossible if sense-percept and sense-image were exactly the same. Hence he denies that "the seat of the ideas is the same as the seat of the sensations," though "of course," he adds; "it is not likely that there will be any wide separation . . . . both might even belong to the same convolution, though possibly to different layers of its cortex." Facts of brain change, especially the "evidence of the gradual maturing of the cerebral projection-system and of its priority to the association-tracts" are adduced to prove the physiological possibility of assimilation, when association is impossible. In his positive treatment of assimilation, Dr. Ward gives, as he says, 'no precise answer to the question,' but this is natural, for his tendency is to regard assimilation as an ultimate, and therefore unanalyzable, aspect of conscious activity. Assimilation is consciousness of familiarity, and the interesting parallel between cognitive familiarity and motor facility, or habit, suggests, at the outset, that the nature of familiarity "is to be formed rather in the subjective than in the objective constituents of consciousness." Quite in consonance with this premonitory remark, assimilation is, in the end, allied with attention of a 'spontaneous, selective and concentrated form,' and described as a form 'of subject activity and interest.'

The most curious feature of the essay, is Dr. Ward's persuasion that his theory is 'in the main' in agreement with Höffding's. In contending that such immediate recognition does exist, without necessarily involving contiguous association, both authors of course agree. But Höffding expressly makes the identification, justly repudiated by Ward, of assimilation with 'association by similarity;' and in the details of his theory, even in the later, modified form of it, he either, as Ward admits, "lays dangerous stress on the physiological effects of mere repetition," or offers a psycho-mechanical explanation differing inherently from Ward's emphatic insistence upon the preëminently subjective nature of assimilation.

Mary Whiton Calkins.

LOGICAL.

History and Natural Science. W. WINDELBAND. Inaugural Address as Rector. Strassburg, May, 1894.

The current division of the experiential sciences into natural sciences and sciences of mind is unfortunate. Psychology does not fall exclusively in either class. For while as regards its object of study it is a 'science of mind,' its method is that of the natural sciences, viz., that of seeking *laws* of processes, whereas most of the 'Geisteswissenschaften' seek to set forth some *single* event or process in its historic relations. A better division from a purely methodological point of view would be into *natural* and *historical* sciences. The former seek universal laws, the latter seek particular historical facts. The goal of the former is the general apodictic judgment, of the latter the singular assertory proposition. The former may be called *nomothetic*, the latter *idiographic*, bearing in mind that the same material may often be considered from either point of view. Logic has always been under the influence of the nomothetic thought forms. It has centered its investigation about the universal judgment, and as regards the actual procedure of science to-day we have devoted far more attention to the theory of experimentation than to the parallel problems of historical methodology.

The results of these diverse methods are widely differing structures. History presents a world of living, concrete individuals; science leaves out all that is individual and constructs her mathematical formulations of the laws of motion, her world of atoms colorless and soundless. This at once suggests the question as to the respective values of the two results, especially as regards their influence on our views of the world and of life. Every object or fact has value in proportion as it contributes to a universal. The particular may fulfil this function, however, not only by being subsumed under a general concept but also by forming a significant constituent of a concrete whole. Man is preëminently interested in the individual and unique, especially in the sphere of personality. But on the other hand the idiographic sciences which portray this must employ general principles furnished by the nomothetic.

These two *moments* of human thought cannot be reduced to one. Causal explanation of historical events might seem to indicate a possibility of completely explaining the particular from universal laws, and so Leibnitz thought that ultimately all *vérités de fait* had their grounds in the *vérités éternelles*. But he could only postulate this for the divine thought, not carry it out for human intelligence. This

'irreducibility' may be illustrated by the fact that every event may be regarded as the conclusion of a syllogism requiring two premises,—one, the major, a natural law, the other the minor, an actual antecedent in time. Hence the individual can never be completely explained, *i. e.*, reduced to general laws. This is illustrated by the impossibility of a complete analysis of personality by general categories, where the irreducible element appears to us as the feeling of the causelessness of our own nature, *i. e.*, individual freedom.

J. H. Tufts.

University of Chicago.

The Conception of Infinity. Shadworth H. Hodgson. Proceedings of the Aristotelian Society, Vol. II., No. 3, Part 1.

It is impossible to analyze and criticise satisfactorily in so brief a space as is contained in the limits of this review, Mr. Hodgson's interesting and closely reasoned paper on infinity. I can only give the outline of his argument and indicate what seem to me its strong and its weak points.

The term *infinity*, Mr. Hodgson maintains, signifies an attribute and not a substance. Since all attributes qualify the substance to which they belong, infinity must be a quality of the thing to which it belongs. But though infinity is thus a quality, it is, so to speak, a *quantitative* quality. In the wider sense of the word it is a quality but not in the narrower. All quantitative determinations of things are qualitative, in the broader sense of the term. There must, then, be something an essential quality of which is quantitative, making it capable of quantification.

The two senses in which the word quality may be used may be brought out in an illustration : we may distinguish in the experience we call a rose : (1) Certain specific feelings such as color, odor, etc. (2) Certain properties which are its quantitative elements, and (3) The fact that certain constituents of these two classes are combined in a particular way at a particular time—the 'existential' element.

The second class, or the quantitative constituents of the experience, are the source of our having the perception or the idea of quantity at all. There are two constituents of this class, time-duration and space-extension. Every content of consciousness must contain a constituent of class (1) and of class (2). The constituents of class (2) may be called the formal element of consciousness. This formal element alone is immediately and essentially capable of quantification, and is the source of all our knowledge of quantity.

Feeling is only quantifiable indirectly, in virtue of its occupying time, or both space and time.

Since, therefore, infinity is a quantitative quality, it is to time and space alone that it must in the last resort attach, if it exists at all ; and time and space are those entities or substances or things whose essential quality is to be quantifiable.

But looking at the panorama of our experience where we will, we find that divisions between feelings, or between feeling and absence of feeling, are always divisions which fall within time or space, never beyond them, *i. e.*, they are limits beyond which there are space and time again, whether this space or time beyond the limit is or is not occupied by a specific feeling or content. In other words space and time in their entirety are wholly limitless and inexhaustible.

Infinity is a fact of perception, observed alike in the minima and in the maxima of perception. Our perception of infinity must be carefully distinguished from our conception of infinity,—the latter being a single finite item in a whole hierarchy of similar conceptions. Our perception of the infinity of space and time is a fact of experience.

If what precedes be true, then we cannot but conceive the universe as infinitely extended. We perceive time and space to be infinite, and as the formal element of consciousness is inseparable from its material co-element ('quality' in the narrower sense), we must conceive existence as extending commensurably with time and space, beyond the boundaries of existence as positively known by us. Time and space, beyond the bounds of any content positively known to us, must be combined with some co-element or other, for it is only as a co-element that we know them.

The universe as positively known or knowable is a world of matter. The unseen world may be immaterial, and faith lays hold upon it, thus satisfying the demands of the moral nature, which positive knowledge leaves unsatisfied. This is the triumph of the Practical Reason, on the field upon which the Speculative has met with defeat.

Such is Mr. Hodgson's argument. I have given it only in abstract and I have eliminated certain portions, but I have not broken the chain of his reasonings. His analysis of consciousness into its formal and material elements is, I think, clear and masterly. His style is simple and without ornament,—a great virtue in a philosophical writer. His conclusion does not appear to me to be proved.

I shall not enter into a general discussion of the infinity and infinite divisibility of space and time, though I think his arguments to prove these are not above reproach, but I shall confine myself to pointing out that consistency would rob him of his world of faith, and shut him up to the world of matter, or such part of it as comes within experience.

He has said that the infinity of time and space are given in experience. Time and space, however, are, as he has also maintained, given in experience only as co-elements,—they are inseparable from the material elements of experience. It would surely seem to follow that if the infinity of time and space are perceptual facts, the infinity of the world in time and space must be a perceptual fact too. Can we have an experience consisting of a certain limited quantity of 'form' and 'matter' combined, and, extending infinitely beyond that, an experience which consists of but the one element, the formal? Are pure space and pure time without any material filling given in perception? If they can be, a world beyond the limits of the known world, a world of faith, cannot be assumed to fill the void, for the void can be conceived as unfilled,—no co-element is necessary. If, on the other hand, a co-element be really necessary, we cannot perceive time and space to be infinite, without at the same time perceiving that which fills them to be infinite, for they cannot be experienced alone.

Now Mr. Hodgson holds that the universe is not positively known to us as infinite. Its infinity is not a perceptual fact. Must we not thence conclude that the infinity of time and space, the formal element in experience, is also not positively known to us? Co-elements must be co-extensive, if they are really inseparable.

Mr. Hodgson must either hold that the formal element may be divorced from the material, in which case he cannot assume the existence of an unknown world to fill void time and space; or that it can not, in which case he must admit that, if we positively know time and space to be infinite, we also positively know the existent world to be infinite. This makes an argument for its existence unnecessary, and rubs out the line between the worlds of knowledge and of faith.

I do not believe Mr. Hodgson can get his conclusion without blowing both hot and cold in his premises. He has certainly done so in this argument.

UNIVERSITY OF PENNSYLVANIA. G. S. F.

L'Abstraction et son rôle dons l'education intellectuelle. QUEYRAT. Paris, Alcan, 1894. Pp. 144.

This is a popular statement of classical ideas on abstraction, ideas to which the author has added, in order to rejuvenate them, many quotations borrowed from contemporary psychologists. The book merits neither praise nor blame; it may be useful to persons entirely unacquainted with psychology. It should serve to confirm others in the idea that those who make science are the ones who should take the trouble to popularize it. A. B.

SORBONNE, PARIS.

ANTHROPOLOGY.

Memoirs of the International Congress of Anthropology. Edited by C. STANILAND WAKE. Chicago, 1894. Pp. 375.

The prompt appearance of this volume (it was issued last spring) and the handsome style of its manufacture secure for it the first words of commendation, and they will certainly not be the last.

At the International Congress, of which it is the record, anthropology was interpreted in its widest sense as the science of man, in all directions of his development, physical and psychical. An effort was made to consider the history of the species as an aggregate, and to lay down the principles for its scientific analysis. The separate branches considered were physical anthropology, archæology, ethnology, folk-lore, religions and linguistics, and in the published volume two or more papers are given on each of these divisions.

While all of them are meritorious, a few deserve special mention on account of the extent of new observations, the results of which they present. In Dr. Franz Boas' article 'The Anthropology of the North American Indian,' and in that of Dr. Gerald M. West on 'The Anthropometry of American School Children,' we have condensed statements of the many thousand measurements undertaken by the Department of Ethnology of the Columbian Exposition. For the first time positive data of wide provenance are supplied in the two branches named.

Another paper replete with new facts is that on 'Primitive Scales and Rhythms,' by Prof. John C. Fillmore. It is a study of the musical powers of the American Indian by one deeply versed in the theory of the art.

Mr. Mercer gives an account of the discovery by himself of a flaked flint in ancient quaternary gravels in Spain; and Prof. O. T. Mason contributes an interesting study on aboriginal American

mechanics. Mrs. S. W. Stevenson illustrates a phase of primitive thought by tracing an ancient Egyptian rite; and Miss Alice C. Fletcher presents some Omaha love songs, showing that this sentiment is also one of those which are primitive. Two of the articles are in German, which is quite appropriate in an 'international' volume.

It is to be regetted that psychology proper did not find a place among the subjects discussed, or at least not among the papers printed. The science of anthropology is not a science when psychology is omitted, and the sooner its commanding position in the study of man is recognized the more rapid will be the progress of a sound knowledge of the species.

D. G. Brinton.
University of Pennsylvania.

EDUCATIONAL AND CHILD PSYCHOLOGY.

The Proceedings of the International Congress of Education of the World's Columbian Exhibition. Held in Chicago, July 25–28, 1893. New York, National Educational Association, 1894. Pp. XVIII. + 1005.

The extended series of volumes representing the proceedings of the National Educational Association since its formation, in 1857, is certainly one of the dreariest collections of pedagogical words and phrases to be found in our language. The volumes will in no way stand comparison with the proceedings of the American Institute of Instruction. The present thick volume of more than a thousand pages goes far, however, toward redeeming the whole series. It gives an encyclopedic view of the educational theories of to-day as presented by some thousands of representative men and women from all over the world in a three days' educational congress. It is not generally understood that there were in Chicago during the summer of 1893 two educational congresses meeting in the same rooms on successive weeks, and each divided into a score of sections covering essentially the same ground. The first congress, July 17th to 25th, was under the direction of the Woman's Branch of the World's Congress Auxiliary, and its proceedings have never been published. The second congress was under the direction of the National Educational Association, July 25th to 28th, and the present volume gives most of the papers presented, with brief notes on the discussions.

One is struck in reading this volume, as in reading all the teachers' literature of the day, with the fact that hardly any of the papers deal with what has been, or with what is; they all struggle with the

question: What ought to be? The exceptions are to be found in the papers of some of the foreign delegates who describe work actually being done in their own countries, and in the work of the psychological sections. It would seem that on a great historical occasion like that which drew the congresses together it might have been expected that some effort would be made to gather up the results of our past pedagogic experience, and then from the historical point of view forecast the probable future. There are very few papers written from this point of view, but most of the discussions are purely theoretical.

The work of the section in Rational Psychology fills some thirty pages, and includes studies by such representative men as Dr. McCosh, Prof. Royce and President Schurman. Cynics who charge us pedagogues with being men of narrow intellectual interests will find sufficient breadth of view in this section to reverse the charge.

Of most interest to readers of this review will be the seventy pages devoted to the work of the Congress of Experimental Psychology in Education. Two years before, at its meeting in Toronto, in 1891, the N. E. A. had given some attention to this line of study through two round-table conferences, presided over by G. Stanley Hall, though they were not recognized as a part of the regular association work. Again, in 1892, at Saratoga, two round-table meetings dealt with experimental psychology, but the meeting in Chicago was really the first great educational meeting in America where any considerable time was set aside for the direct study of the original stuff with which all educational theory deals. The papers at Chicago present special studies on physical development, stuttering, imagination in childhood, children's language, children's theology, eye and ear-mindedness, the psychology of reading and spelling, reports on work being done in different parts of the world, with several more general studies showing the relation of this sort of work to educational theory and practice. Such well-known men are represented as G. Stanley Hall, Wm. Burnham, James Sully, E. M. Hartwell, Francis Warner, and Wm. Bryan. There is no other single place where so much material has been brought together bearing on the study of children as in this volume, with the exception of the files of the Pedagogical Seminary.

The volume must prove to be a valuable and permanent reference book for students interested in educational theory and the beginnings of educational psychology.

EARL BARNES.
STANFORD UNIVERSITY.

NEW BOOKS.

Lehrbuch der Psychologie. W. Volkmann. Vierte Auf., Bd. I. Cöthen, Schulze, 1894. Pp. vii + 511.

Peregrinazioni psicologiche. Tr. Vignoli. Milan, Hoepli, 1895. Pp. 404.

Filosofia morale. L. Friso. Milan, Hoepli, 1893. Pp. xii + 335.

From the Greeks to Darwin. H. F. Osborn. New York, Macmillan & Co., and London, 1894. Pp. x + 259.

Lectures on Human and Animal Psychology. W. Wundt. Translated from the second German edition by E. Creighton and E. B. Titchener. London, Swan, Sonnenschein & Co.; New York, Macmillan & Co., 1894. Pp. x + 454.

Saggio di una scala normale del pensiero astratto. S. de Crescento. Naples, d'Auria, 1893. Pp. 27.

Principii di logica reale. N. R. d'Alfonso. Rome, Paravia, 1894. Pp. 71.

Genie und Entartung: eine psychologische Studie. W. Hirsch. Berlin u. Leipzig, Coblenz, 1894.

Zur Analyse des Apperceptionsbegriffs. J. Kodis. Berlin, Calvary, 1893. Pp. 202.

Introduction to Comparative Psychology. C. Lloyd Morgan. Contemporary Science Series. London, Walter Scott; New York, imported by Charles Scribner's Sons, 1894. Pp. xiv, 382.

Entwurf zu einer physiologischen Erklarung der psychischen Erscheinungen. I. Thl. Sigm. Exner. Vienna, Deuticke, 1894. Pp. viii + 380.

Studies from the Yale Psychological Laboratory. II. Edited by Edward W. Scripture. New Haven, Conn., Yale University, 1894. Pp. 124. $1.

La logique sociale. G. Tarde. Paris, Alcan, 1894. 7 fr. 50.

Mémoire et imagination. Lucien Arréat. Paris, Alcan, 1894. 2 fr. 50.

Les états intellectuels dans la mélancolie. G. Dumas. Paris, Alcan, 1895. Pp. 144. 2 fr. 50.

Les lois psychologiques de l'évolution des peuples. G. Le Bon. Paris, Alcan, 1894. 2 fr. 50.

NOTES.

Ex-President James McCosh of Princeton College died in Princeton on Nov. 16.

Professor O. Külpe has been called from Leipzig to the University of Wurzburg.

Professor Sorley has been called to the University of Aberdeen.

The publication is announced of Prof. Ribot's lectures at the *College de France*. The volumes, of which there will probably be three, are now in press for early issue.

Mr. A. H. Lloyd has been made Acting Assistant Professor of Philosophy, and given charge of the department for the year, in the University of Michigan. Mr. J. Bigham, Ph.D. (Harvard), and Mr. Geo. Rebec, Ph.B. (Michigan), have been appointed instructors in the same institution, the former to direct the work of the psychological laboratory.

Dr. W. R. Newbold has been appointed Assistant Professor of Philosophy in the University of Pennsylvania.

Dr. S. Mezes has been appointed Professor of Philosophy in the University of Texas.

Dr. Margaret Washburn has been appointed Professor of Philosophy and Psychology in Wells College.

Dr. W. B. Elkin has been appointed Professor of Philosophy in Colgate University.

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THE PSYCHOLOGICAL REVIEW.

THE KNOWING OF THINGS TOGETHER.[1]

BY PROFESSOR WILLIAM JAMES,

Harvard University.

I.

The nature of the synthetic unity of consciousness is one of those great underlying problems that divide the psychological schools. We know, say, a dozen things singly through a dozen different mental states. But on another occasion we may know the same dozen things together through a single mental state. The problem is as to the relation of the previous many states to the later one state. In physical nature, it is universally agreed, a multitude of facts always remain the multitude they were and appear as one fact only when a mind comes upon the scene and so views them, as when H–O–H appear as 'water' to a human spectator. But when, instead of extramental 'things,' the mind combines its own 'contents' into a unity, what happens is much less plain.

The matters of fact that give the trouble are among our most familiar experiences. We know a lot of friends and can think of each one singly. But we can also think of them together, as composing a 'party' at our house. We can see single stars appearing in succession between the clouds on a stormy night, but we can also see whole constellations of those stars at once when the wind has blown the clouds away. In a glass of lemonade we can taste both

[1] Read as the President's Address before the American Psychological Association at Princeton, December, 1894, and reprinted with some unimportant omissions, a few slight revisions, and the addition of some explanatory notes.

the lemon and the sugar at once. In a major chord our ear can single out the *c*, *e*, *g*, and *c'*, if it has once become acquainted with these notes apart. And so on through the whole field of our experience, whether conceptual or sensible. Neither common sense nor commonplace psychology finds anything special to explain in these facts. Common sense simply says the mind 'brings the things together,' and common psychology says the 'ideas' of the various things 'combine,' and at most will admit that the occasions on which ideas combine may be made the subject of inquiry. But to formulate the phenomenon of knowing things together thus as a combining of ideas, is already to foist in a theory about the phenomenon simply. Not so should a question be approached. The phenomenon offers itself, in the first instance, as that of *knowing things together;* and it is in those terms that its solution must, in the first instance at least, be sought.

'Things,' then; to 'know' things; and to know the 'same' things 'together' which elsewhere we knew singly —here, indeed, are terms concerning each of which we must put the question, 'What do we *mean* by it when we use it?'—that question that Shadworth Hodgson lays so much stress on, and that is so well taught to students, as the beginning of all sound method, by our colleague Fullerton. And in exactly ascertaining what we do mean by such terms there might lie a lifetime of occupation.

For we do mean something; and we mean something true. Our terms, whatever confusion they may connote, denote at least a fundamental fact of our experience, whose existence no one here present will deny.

II.

What, then, do we mean by 'things'? To this question I can only make the answer of the idealistic philosophy. For the philosophy that began with Berkeley, and has led up in our tongue to Shadworth Hodgson, things have no other nature than thoughts have, and we know of no things that are not given to somebody's experience. When I see

the thing white paper before my eyes, the nature of the thing and the nature of my sensations are one. Even if with science we supposed a molecular architecture beneath the smooth whiteness of the paper, that architecture itself could only be defined as the stuff of a farther possible experience, a vision, say, of certain vibrating particles with which our acquaintance with the paper would terminate if it were prolonged by magnifying artifices not yet known. A thing may be my phenomenon or some one else's; it may be frequently or infrequently experienced; it may be shared by all of us; one of our copies of it may be regarded as the original, and the other copies as representatives of that original; it may appear very differently at different times; but whatever it be, the stuff of which it is made is thought-stuff, and whenever we speak of a thing that is out of our own mind, we either mean nothing; or we mean a thing that was or will be in our own mind on another occasion; or, finally, we mean a thing in the mind of some other possible receiver of experiences like ours.

Such being 'things,' what do we mean by saying that we 'know' them?

There are two ways of knowing things, knowing them immediately or intuitively, and knowing them conceptually or representatively. Although such things as the white paper before our eyes can be known intuitively, most of the things we know, the tigers now in India, for example, or the scholastic system of philosophy, are known only representatively or symbolically.

Suppose, to fix our ideas, that we take first a case of conceptual knowledge; and let it be our knowledge of the tigers in India, as we sit here. Exactly what do we *mean* by saying that we here know the tigers? What is the precise fact that the cognition so confidently claimed is *known-as*, to use Shadworth Hodgson's inelegant but valuable form of words?

Most men would answer that what we mean by knowing the tigers is having them, however absent in body, become in some way present to our thought; or that our knowledge of them is known as presence of our thought to them. A great mystery is usually made of this peculiar presence in

absence; and the scholastic philosophy, which is only common sense grown pedantic, would explain it as a peculiar kind of existence, called *intentional inexistence*, of the tigers in our mind. At the very least, people would say that what we mean by knowing the tigers is mentally *pointing* towards them as we sit here.

But now what do we mean by *pointing*, in such a case as this? What is the pointing known-as, here?

To this question I shall have to give a very prosaic answer—one that traverses the prepossessions not only of common sense and scholasticism, but also those of nearly all the epistemological writers whom I have ever read. The answer, made brief, is this: The pointing of our thought to the tigers is known simply and solely as a procession of mental associates and motor consequences that follow on the thought, and that would lead harmoniously, if followed out, into some ideal or real context, or even into the immediate presence, of the tigers. It is known as our rejection of a jaguar, if that beast were shown us as a tiger; as our assent to a genuine tiger if so shown. It is known as our ability to utter all sorts of propositions which don't contradict other propositions that are true of the real tigers. It is even known, if we take the tigers very seriously, as actions of ours which may terminate in directly intuited tigers, as they would if we took a voyage to India for the purpose of tiger-hunting and brought back a lot of skins of the striped rascals which we had laid low. In all this there is no self-transcendency in our mental images taken by themselves. They are one physical fact; the tigers are another; and their pointing to the tigers is a perfectly commonplace physical relation, if you once grant a connecting world to be there. In short, the ideas and the tigers are in themselves as loose and separate, to use Hume's language, as any two things can be; and pointing means here an operation as external and adventitious as any that nature yields.[1]

[1] A stone in one field may 'fit,' we say, a hole in another field. But the relation of 'fitting,' so long as no one carries the stone to the hole and drops it in, is only one name for the fact that such an act may happen. Similarly with the knowing of the tigers here and now. It is only an anticipatory name for a further associative and terminative process that may occur.

I hope you may agree with me now that in representative knowledge there is no special inner mystery, but only an outer chain of physical or mental intermediaries connecting thought and thing. *To know an object is here to lead to it through a context which the world supplies.* All this was most instructively set forth by our colleague Miller, of Bryn Mawr, at our meeting in New York last Christmas, and for re-confirming my sometime wavering opinion, I owe him this acknowledgment.[1]

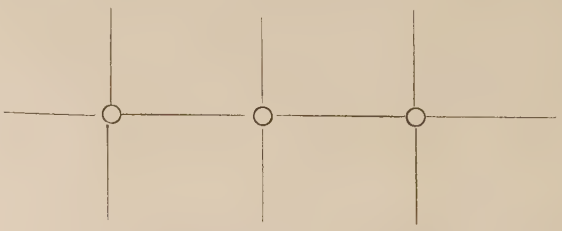
Let us next pass on to the case of immediate or intuitive acquaintance with an object, and let the object be the white paper before our eyes. The thought-stuff and the thing-stuff are here indistinguishably the same in nature, as we saw a moment since, and there is no context of intermediaries or associates to stand between and separate the thought and thing. There is no 'presence in absence' here, and no 'pointing,' but rather an allround embracing of the paper by the thought; and it is clear that the knowing cannot now be explained exactly as it was when the tigers were its object. Dotted all through our experience are states of immediate acquaintance just like this. Somewhere our belief always does rest on ultimate data like the whiteness, smoothness, or squareness of this paper. Whether such qualities be truly ultimate aspects of being or only provisional suppositions of ours, held-to till we get better informed, is quite immaterial for our present inquiry. So long as it is believed in, we see our object face to face. What now do we mean by 'knowing' such a sort of object as this? For this is also the way in which we should know the tiger if our conceptual idea of him were to terminate by having led us to his lair?

This address must not become too long, so I must give my answer in the fewest words. And let me first say this: So far as the white paper or other ultimate datum of our experience is considered to enter also into some one else's experience, and we, in knowing it, are held to know it there as well as here; so far again as it is considered to be a mere

[1] See also Dr. Miller's article on Truth and Error, in the *Philosophical Review*, July, 1893.

mask for hidden molecules that other now impossible experiences of our own might some day lay bare to view; so far it is a case of tigers in India again—the things known being absent experiences, the knowing can only consist in passing smoothly towards them through the intermediary context that the world supplies. But if our own private vision of the paper be considered in abstraction from every other event, as if it constituted by itself the universe (and it might perfectly well do so, for aught we can understand to the contrary), then the paper seen and the seeing of it are only two names for one indivisible fact which, properly named, is *the datum, the phenomenon, or the experience.* The paper is in the mind and the mind is around the paper, because paper and mind are only two names that are given later to the one experience, when, taken in a larger world of which it forms a part, its connections are traced in different directions.[1] *To know immediately, then, or intuitively, is for mental content and object to be identical.* This is a very different definition from that which we gave of representative knowledge; but neither definition involves those mysterious notions of self-transcendency and presence in absence which are such essential parts of the ideas of knowledge, both of common men and of

[1] What is meant by this is that 'the experienee' can be referred to either of two great associative systems, that of the experiencer's mental history, or that of the experienced facts of the world. Of both of these systems it forms part, and may be regarded, indeed, as one of their points of intersection. One might let a vertical line



stand for the mental history; but the same object, O, appears also in the mental history of different persons, represented by the other vertical lines. It thus ceases to be the private property of one experience, and becomes, so to speak, a shared or public thing. We can track its outer history in this way, and represent it by the horizontal line. [It is also known representatively at other points of the vertical lines, or intuitively there again, so that the line of its outer history would have to be looped and wandering, but I make it straight for simplicity's sake.] In any case, however, it is the same *stuff* that figures in all the sets of lines.

philosophers. Is there no experience that can justify these notions, and show us somewhere their original?

I think the mystery of presence in absence (though we fail to find it between one experience and another remote experience to which it points, or between the 'content' and 'object' of any one experience falsely rent asunder by the application to it of these two separate names) may yet be found, and found between the parts of a single experience. Let us look for it, accordingly, in its simplest possible form. What is the smallest experience in which the mystery remains? If we seek, we find that there is no datum so small as not to show the mystery. The smallest effective pulse of consciousness, whatever else it may be consciousness of, is also consciousness of passing time. The tiniest feeling that we can possibly have involves for future reflection two sub-feelings, one earlier and the other later, and a sense of their continuous procession. All this has been admirably set forth by Mr. Shadworth Hodgson,[1] who shows that there is literally no such datum as that of the present moment, and no such content, and no such object, except as an unreal postulate of abstract thought. The *passing* moment is the only thing that ever concretely was or is or shall be; and in the phenomenon of elementary memory, whose function is to apprehend it, earlier and later are present to each other in an experience that feels either only on condition of feeling both together.

We have the same knowing together in the matter that fills the time. The rush of our thought forward through its fringes is the everlasting peculiarity of its life. We realize this life as something always off its balance, something in transition, something that shoots out of a darkness through a dawn into a brightness that we know to be the dawn fulfilled. In the very midst of the alteration our experience comes as one continuous fact. 'Yes,' we say at the moment of full brightness, *this* is what I meant. No, we feel at the moment of the dawning, this is not yet the meaning, there is more to come. In every crescendo of sensation, in every effort

[1] *Philosophy of Reflection*, Vol. I, p. 248 ff.

to recall, in every progress towards the satisfaction of desire, this succession of an emptiness and fulness that have reference to each other and are one flesh is the essence of the phenomenon. In every hindrance of desire the sense of ideal presence of what is absent in fact, of an absent, in a word, which the only function of the present is to *mean*, is even more notoriously there. And in the movement of thoughts not ordinarily classed as involving desire, we have the same phenomenon. When I say *Socrates is mortal*, the moment *Socrates* is incomplete; it falls forward through the *is* which is pure movement, into the *mortal*, which is indeed bare mortal on the tongue, but for the mind, is *that mortal*, the *mortal Socrates*, at last satisfactorily disposed of and told off.

Here, then, inside of the minimal pulse of experience which, taken as object, is change of feeling, and, taken as content, is feeling of change, is realized that absolute and essential self-transcendency which we swept away as an illusion when we sought it between a content taken as a whole and a supposed objective thing outside. *Here in the elementary datum of which both our physical and our mental worlds are built, we find included both the original of presence in absence and the prototype of that operation of knowing many things together which it is our business to discuss.*[1]

[1] It seems to me that we have here something like what comes before us in the psychology of space and time. Our original intuition of space is the single field of view; our original intuition of time covers but a few seconds; yet by an ideal piecing together and construction we frame the notions of immensity and eternity, and suppose dated events and located things therein, of whose actual intervals we grasp no distinct idea. So in the case before us. The way in which the constituents of one undivided datum drag each other in and run into one, saying *this* is what *that* means, gives us our original intuition of what knowing is. That intuition we extend and constructively build up into the notion of a vast tissue of knowledge, shed along from experience to experience until, dropping the intermediary data from our thought, we assume that terms the most remote still know each other, just after the fashion of the parts of the prototypal fact. Cognition here is only constructive, as we have already seen. But he who should say, arguing from its nature here, that it nowhere is direct, and seek to construct it without an originally given pattern, would be like those psychologists who profess to develop our idea of space out of the association of data that possess no original extensity. Grant the *sort* of thing that is meant by presence in absence, by self-transcendency, by reference to another, by pointing forward or back, by knowledge in short, somewhere in our experience, be it in ever so small a corner, and the construction of pseudo-cases elsewhere follows as a matter of course. But to get along without the real thing *anywhere* seems difficult indeed.

For the fact that past and future are already parts of the least experience that can really be, is just like what we find in any other case of an experience whose parts are many. Most of these experiences are of objects perceived to be simultaneous and not to be immediately successive as in the heretofore considered case. The field of view, the chord of music, the glass of lemonade are examples. But the gist of the matter is the same—it is always knowing-together. You cannot separate the consciousness of one part from that of all the rest. What is given is pooled and mutual; there is no dark spot, no point of ignorance; no one fraction is eclipsed from any other's point of view. Can we account for such a being-known-together of complex facts like these?

The general *nature* of it we can probably never account for, or tell how such a unity in manyness can be, for it seems to be the ultimate essence of all experience, and anything less than it apparently cannot be at all. But the particular *conditions* whereby we know particular things together might conceivably be traced, and to that humble task I beg leave to devote the time that remains.

III.

Let me say forthwith that I have no pretension to give any positive solution. My sole ambition now is, by a little classification, to smooth the ground somewhat so that some of you, more able than I, may be helped to advance, before our next meeting perhaps, to results that I cannot obtain.

Now, the first thing that strikes us in these complex cases is that the condition by which one thing may come to be known together with other things is an *event*. It is often an event of the purely physical order. A man walks suddenly into my field of view, and forthwith becomes part of it. I put a drop of cologne-water on my tongue, and, holding my nostrils, get the taste of it alone, but when I open my nostrils I get the smell together with the taste in mutual suffusion. Here it would seem as if a sufficient condition of the knowing of (say) three things together were the fact that the three several physical conditions of the

knowing of each of them were realized at once. But in many other cases we find on the contrary that the physical conditions are realized without the things being known together at all. When absorbed in experiments with the cologne-water, for example, the clock may strike, and I not know that it has struck. But again, some seconds after the striking has elapsed, I may, by a certain shifting of what we call my attention, hark back to it and resuscitate the sound, and even count the strokes in memory. The condition of knowing the clock's striking is here an event of the mental order which must be added to the physical event of the striking before I can know it and the cologne-water at once. Just so in the field of view I may entirely overlook and fail to notice even so important an object as a man, until the inward event of altering my attention makes me suddenly see him with the other objects there. In those curious phenomena of dissociation of consciousness with which recent studies of hypnotic, hysteric and trance-states have made us familiar (phenomena which surely throw more new light on human nature than the work of all the psycho-physical laboratories put together), the event of hearing a 'suggestion,' or the event of passing into trance or out of it, is what decides whether a human figure shall appear in the field of view or disappear, and whether a whole set of memories shall come before the mind together, along with its other objects, or be excluded from their company. There is in fact no possible object, however completely fulfilled may be the outer condition of its perception, whose entrance into a given field of consciousness does not depend on the additional inner event called attention.

Now, it seems to me that this need of a final inner event, over and above the mere sensorial conditions, quite refutes and disposes of the associationist theory of the unity of consciousness. By associationist theory, I mean any theory that says, either implicitly or explicitly, that for a lot of objects to be known together, it suffices that a lot of conscious states, each with one of them as its content, should exist, as James Mill says, 'synchronically.' Synchronical existence of the ideas does not suffice, as the facts we now

have abundantly show. Gurney's, Binet's and Janet's proofs of several dissociated consciousnesses existing synchronically, and dividing the subject's field of knowledge between them, is the best possible refutation of any such view.

Union in consciousness must be *made* by something, must be brought about; and to have perceived this truth is the great merit of the anti-associationist psychologists.[1] The form of unity, they have obstinately said, must be specially accounted for; and the form of unity the radical associationists have as obstinately shied away from and ignored, though their accounts of those preliminary conditions that supply the matters to be united have never been surpassed. As far as these go, we are all, I trust, associationists, and reverers of the names of Hartley, Mill, and Bain.

Let us now rapidly review the chief attempts of the anti-associationists to fill the gap they discern so well in the associationist tale.

1. *Attention.*—Attention, we say, by turning to an object, includes it with the rest; and the naming of this faculty in action has by some writers been considered a sufficient account of the decisive 'event.'[2] But it is plain that the act of Attention itself needs a farther account to be given, and such an account is what other theories of the event implicitly give.

We find four main types[3] of other theory of how par-

[1] In this rapid paper I content myself with arguing from the experimental fact that something *happens* over and above the realization of sensorial conditions, wherever an object adds itself to others already 'before the mind.' I say nothing of the logical self-contradiction involved in the associationist doctrine that the two facts, 'A is known,' and 'B is known,' *are* the third fact, 'A + B are known together.' Those whom the criticisms already extant in print of this strange belief have failed to convince, would not be persuaded, even though one rose from the dead. The appeal to the actual facts of dissociation may make impression, however, even on such hardened hearts as theirs.

[2] It might seem natural to mention Wundt's doctrine of 'Apperception' here. But I must confess my inability to say anything about it that would not resolve itself into a tedious comparison of texts. Being alternately described as intellection, will, feeling, synthesis, analysis, principle and result, it is too 'protean' a function to lend itself to any simplified account at second hand.

[3] It is only for the sake of completeness that we need mention such notions of a sort of mechanical and chemical activity between the ideas as we find in Herbart,

ticular things get known together, a physiological, a psychological, an animistic, and a transcendentalist type. Of the physiological or 'psycho-physical' type many varieties are possible, but it must be observed that none of them pretends to assign anything more than an empirical law. A psycho-physical theory can couple certain antecedent conditions with their result; but an explanation, in the sense of an inner reason why the result should have the nature of one content with many parts instead of some entirely different nature, is what a psycho-physical theory cannot give.[1]

2. *Reminiscence.*—Now, empirically, we have learned that things must be known in succession and singly before they can be known together.[2] If A, B, and C, for example, were outer things that came for the first time and affected our senses all at once, we should get one content from the lot of them and make no discriminations. The content would symbolically point to the objects A, B, C, and eventually terminate there, but would contain no parts that were immediately apprehended as standing for A, B, and C severally. Let A, B, and C stand for pigments, or for a tone and its overtones, and you will see what I mean when I say that the first result on consciousness of their falling together on the eye or ear would be a single new

Steinthal and others. These authors see clearly that mere synchronical existence is not combination, and attribute to the ideas dynamic influences upon each other; pressures and resistances according to Herbart, and according to Steinthal 'psychic attractions.' But the philosophical foundation of such physical theories have been so slightly discussed by their authors that it is better to treat them only as rhetorical metaphors and pass on. Herbart, moreover, must also be mentioned later, along with the animistic writers.

[1] We find this impotence already when we seek the conditions of the passing pulse of consciousness, which, as we saw, always involves time and change. We account for the passing pulse, physiologically, by the overlapping of dying and dawning brain-processes; and at first sight the elements time and change, involved in both the brain-processes and their mental result, gives a similarity that, we feel, might be the real reason for the psycho-physic coupling. But the moment we ask 'metaphysical' questions—"Why not each brain-process felt apart?—Why just this amount of time, neither more nor less?" etc., etc.—we find ourselves falling back on the empirical view as the only safe one to defend.

[2] The latest empirical contribution to this subject, with which I am acquainted, is Dr. Herbert Nichols' excellent little monograph, 'Our Notions of Number and Space.' Boston, Ginn & Co., 1894.

kind of feeling rather than a feeling with three kinds of inner part. Such a result has been ascribed to a 'fusion' of the three feelings of A, B, and C; but there seems no ground for supposing that, under the conditions assumed, these distinct feelings have ever been aroused at all. I should call the phenomenon one of *indiscriminate knowing together*, for the most we can say under the circumstances is that the content resembles somewhat each of the objects A, B, and C, and knows them each potentially, knows them, that is, by possibly leading to each smoothly hereafter, as we know Indian tigers even whilst sitting in this room.

But if our memory possess stored-up images of former A-s, B-s, and C-s, experienced in isolation, we get an altogether different content, namely, one through which we know A, B, and C together, and yet know each of them in discrimination through one of the content's own parts. This has been called a 'colligation' or *Verknüpfung* of the 'ideas' of A, B, and C, to distinguish it from the aforesaid fusion. Whatever we may call it, we see that its physiological condition is more complex than in the previous case. In both cases the outer objects, A, B, and C, exert their effects on the sensorium. But in this case there is a coöperation of higher tracts of memory which in the former case was absent. *Discriminative knowing-together, in short, involves higher processes of reminiscence.* Do these give the element of manyness, whilst the lower sensorial processes that by themselves would result in mere 'fusion,' give the unity to the experience? The suggestion is one that might repay investigation, although it has against it two pretty solid objections: first, that in man the consciousness attached to infra-cortical centres is altogether subliminal, if it exist; and, second, that in the cortex itself we have not yet discriminated sensorial from ideational processes. Possibly the frontal lobes, in which Wundt has supposed an *Apperceptionsorgan*, might serve a turn here. In any case it is certain that, into our present rough notions of the cortical functions, the future will have to weave distinctions at present unknown.

3. *Synergy.*—The theory that, physiologically, the oneness precedes the manyness, may be contrasted with a theory that our colleagues Baldwin and Münsterberg are at present working out, and which places the condition of union of many data into one datum, in the fact that the many pour themselves into one motor discharge. The motor discharge being the last thing to happen, the condition of manyness would physiologically here precede and that of oneness follow. A printed word is apprehended as one object, at the same time that each letter in it is apprehended as one of its parts. Our secretary, Cattell, long ago discovered that we recognize words of four or five letters by the eye as quickly, or even more quickly, than we recognize single letters. Recognition means here the motor process of articulation; and the quickness comes from the fact that all the letters in the particular combination unhesitatingly coöperate in the one articulatory act. I suppose such facts as these to lie at the base of our colleagues' theories, which probably differ in detail, and which it would be manifestly unjust to discuss or guess about in advance of their completer publication. Let me only say that I hope the latter may not be long delayed.

These are the only types of physiological theory worthy of mention. I may next pass to what, for brevity's sake, may be called *psychological accounts* of the event that lets an object into consciousness, or, by not occurring, leaves it out. These accounts start from the fact that what figures as part of a larger object is often perceived to have relations to the other parts. Accordingly the event in question is described as an *act of relating thought*. It takes two forms.

4. *Relating to Self.*—Some authors say that nothing can enter consciousness except on condition that it be related to the self. Not *object*, but *object-plus-me*, is the minimum knowable.

5. *Relating to other Objects.*—Others think it enough if the incoming object be related to the other objects already there. To fail to appear related is to fail to be known at all. To appear related is to appear with other objects. If relations were correlates of special cerebral processes, the

addition of these to the sensorial processes would be the wished-for event. But brain physiology as yet knows nothing of such special processes, so I have called this explanation purely psychological. There seem to be fatal objections to it as a universal statement, for the reference to self, if it exist, must in a host of cases be altogether subconscious; and introspection assures us that in many half-waking and half-drunken states the relations between things that we perceive together may be of the dimmest and most indefinable kind.

6. *The Individual Soul.*—So we next proceed to *the animistic account.* By this term I mean to cover every sort of individualistic soul-theory. I will say nothing of older opinions; but in modern times we have two views of the way in which the union of a many by a soul occurs. For Herbart, for example, it occurs because the soul itself *is* unity, and all its *Selbsterhaltungen* are obliged to necessarily share this form. For our colleague Ladd, on the other hand, to take the best recent example, it occurs because the soul, which *is* a real unity indeed, furthermore performs a unifying *act* on the naturally separate data of sense —an act, moreover, for which no psycho-physical analogon can be found. It must be admitted that much of the reigning bias against the soul in so-called scientific circles is an unintelligent prejudice, traceable far more to a vague impression that it is a theological superstition than to exact logical grounds. The soul is an 'entity,' and, indeed, that worst sort of entity, a 'scholastic entity;' and, moreover, it is something to be damned or saved; so let's have no more of it! I am free to confess that in my own case the antipathy to the Soul with which I find myself burdened is an ancient hardness of heart of which I can frame no fully satisfactory account even to myself. I passively agree that if there were Souls that we could use as principles of explanation, the *formal* settlement of the questions now before us could run far more smoothly towards its end. I admit that a soul is a medium of union, and that brain-processes and ideas, be they never so 'synchronical,' leave all mediating agency out. Yet, in spite of these concessions, I never find

myself actively taking up the soul, so to speak, and making it do work in my psychologizing. I speak of myself here because I am one amongst many, and probably few of us can give adequate reasons for our dislike. The more honor to our colleague from Yale, then, that he remains so unequivocally faithful to this unpopular principle! And let us hope that his forthcoming book may sweep what is blind in our hostility away.[1]

But all is not blind in our hostility. When, for example, you say that A, B, and C, which are distinct contents on other occasions, are now on this occasion joined into the compound content ABC by a unifying act of the soul, you say little more than that now they *are* united, unless you give some hint as to *how* the soul unites them. When, for example, the hysteric women which Pierre Janet has studied with such loving care, go to pieces mentally, and their souls are unable any longer to connect the data of their experience together, though these data remain severally conscious in dissociation, what is the condition on which this inability of the soul depends? Is it an impotence in the soul itself? or is it an impotence in the physiological conditions, which fail to stimulate the soul sufficiently to its synthetic task? The *how* supposes on the Soul's part a constitution adequate to the act. An hypothesis, we are told in the logic-books, ought to propose a being that has some other constitution and definition than that of barely performing the phenomenon it is evoked to explain. When physicists propose the 'ether,' for example, they propose it with a lot of incidental properties. But the soul proposed to us has no special properties or constitution of which we are informed. Nev-

[1] I ought, perhaps, to apologize for not expunging from my printed text these references to Professor Ladd, which were based on the impression left on my mind by the termination of his Physiological Psychology. It would now appear from the paper read by him at the Princeton meeting, and his 'Philosophy of Mind,' just published, that he disbelieves in the Soul of old-fashioned ontology; and on looking again at the P. P., I see that I may well have misinterpreted his deeper meaning there. I incline to suspect, however, that he had himself not fully disentangled it when that work was written; and that between now and then his thought has been evolving somewhat, as Lotze's did, between his 'Medical Psychology' and his 'Metaphysic.' It is gratifying to note these converging tendencies in different philosophers; but I leave the text as I read it at Princeton, as a mark of what one could say not so very unnaturally at that date.

ertheless, since particular conditions do determine its activity, it must have a constitution of some sort. In either case, we ought to know the facts. But the soul-doctrine, as hitherto professed, not only doesn't answer such questions, it doesn't even ask them; and it must be radically rejuvenated if it expects to be greeted again as a useful principle in psychological philosophy. Here is work for our spiritualist colleagues, not only for the coming year, but for the rest of their lives.[1]

7. *The World-soul.*—The second spiritualist theory may be named as that of *transcendentalism*. I take it typically and not as set forth by any single author. Transcendentalism explains things by an over-soul of which all separate souls, sensations, thoughts, and data generally are parts. To be, as it would be known together with everything else in the world by this over-soul, is for transcendentalism the *true* condition of each single thing, and to pass into this condition is for things to fulfill their vocation. Such being known together, since it is the innermost reality of life, cannot on transcendentalist principles be explained or accounted for as a work wrought on a previous sort of reality. The monadic soul-theory starts with separate sensational data, and must show how they are *made* one. The transcendentalist theory has rather for its task to show how, being one, they can spuriously and illusorily be made to appear separate. The problem for the monadic soul, in short, is that of unification, and the problem for the over-soul is that of insulation. The removal of insulating obstruc-

[1] The soul can be taken in three ways as a unifying principle. An already existing lot of animated sensations (or other psychic data) may be simply *woven* into one by it; in which case the form of unity is the soul's only contribution, and the original stuff of the Many remains in the One as its stuff also. Or, secondly, the resultant synthetic One may be regarded as an immanent *reaction* of the Soul on the preëxisting psychic Many; and in this case the Soul, in addition to creating the new form, reproduces in itself the old stuff of the Many, superseding it for our use, and making it for us become subliminal, but not suppressing its existence. Or, thirdly, the One may again be the Soul's immanent reaction on a physiological, not on a mental, Many. In this case preëxisting *sensations or ideas* would not be there at all, to be either woven together or superseded. The synthetic One would be a primal psychic datum with parts, either of which might know the same object that a possible sensation, realized under other physiological conditions, could also know.

tions would sufficiently account for things reverting to their natural place in the over-soul and being known together. The most natural insulating or individualizing principle to invoke is the bodily organism. As the pipes of an organ let the pressing mass of air escape only in single notes, so do our brains, the organ pipes of the infinite, keep back everything but the slender threads of truth to which they may be pervious. As they obstruct more, the insulation increases, as they obstruct less it disappears. Now transcendental philosophers have as a rule not done much dabbling in psychology. But one sees no abstract reason why they might not go into psychology as fully as any one, and erect a psycho-physical science of the conditions of more separate and less separate cognition which would include all the facts that psycho-physicists in general might discover. And they would have the advantage over other psycho-physicists of not needing to explain the nature of the resultant knowing-together when it should occur, for they could say that they simply begged it as the ultimate nature of the world.

This is as broad a disjunction as I can make of the different ways in which men have considered the conditions of our knowing things together. You will agree with me that I have brought no new insight to the subject, and that I have only gossiped to while away this unlucky presidential hour to which the constellations doomed me at my birth. But since gossip we have had to have, let me make the hour more gossipy still by saying a final word about the position taken up in my own *Principles of Psychology* on the general question before us, a position which, as you doubtless remember, was so vigorously attacked by our colleague from the University of Pennsylvania at our meeting in New York a year ago.[1] That position consisted in this, that I proposed to simply eliminate from psychology 'considered as a natural science' the whole business of ascertaining *how* we come to know things together or to know them at all. Such considerations, I said, should fall to metaphysics. That we do know

[1] Printed as an article entitled 'The Psychological Standpoint,' in this REVIEW, Vol. I, p. 113. (March, 1894.)

things, sometimes singly and sometimes together, is a fact. That states of consciousness are the vehicle of the knowledge, and depend on brain states, are two other facts. And I thought that a natural science of psychology might legitimately confine itself to tracing the functional variations of these three sorts of fact, and ascertaining and tracing what determinate bodily states are the condition when the states of mind know determinate things and groups of things. Most states of mind can be designated only by naming what objects they are 'thoughts-of,' *i. e.*, what things they know.

Most of those which know compound things are utterly unique and solitary mental entities demonstrably different from any collection of simpler states to which the same objects might be singly known.[1] Treat them all as unique in entity, I said then; let their complexity reside in their plural cognitive function; and you have a psychology which, if it doesn't ultimately explain the facts, also does not, in ex-

[1] When they know conceptually they don't even remotely resemble the simpler states. When they know intuitively they resemble, sometimes closely, sometimes distantly, the simpler states. The sour and sweet in lemonade are extremely unlike the sour and sweet of lemon juice and sugar, singly taken, yet like enough for us to 'recognize' these 'objects' in the compound taste. The several objective 'notes' recognized in the chord sound differently and peculiarly there. In a motley field of view successive and simultaneous contrast give to each several tint a different hue and luminosity from that of the 'real' color into which it turns when viewed without its neighbors by a rested eye. The difference is sometimes so slight, however, that we overlook the 'representative' character of each of the parts of a complex content, and speak as if the latter were a cluster of the original 'intuitive' states of mind that, occurring singly, know the 'object's' several parts in separation. Prof. Meinong, for example, even after the true state of things had been admirably set forth by Herr H. Cornelius (in the *Vierteljahrschrift f. wiss. Phil.*, XVI, 404; XVII, 30), returns to the defence of the radical associationist view (in the *Zeitschrift f. Psychologie*, VI, 340, 417). According to him, the single sensations of the several notes lie unaltered in the chord-sensations; but his analysis of the phenomenon is vitiated by his non-recognition of the fact that the *same objects* (*i. e.*, the notes) *can be known* representatively through one compound state of mind, and directly in several simple ones, without the simple and the compound states having strictly anything in common with each other. In Meinong's earlier work, Ueber Begriff und Eigenschaften der Empfindung (*Vierteljahrschrift*, vol. XII), he seems to me to have hit the truth much better, when he says that the aspect *color*, *e. g.*, in a concrete sensation of *red*, is not an abstractable *part* of the sensation, but an *external relation of resemblance* between that sensation and other sensations to the whole lot of which we give the name of colors. Such, I should say, are the aspects of *c*, *e*, *g* and *c'* in the chord. We may call them *parts* of the chord if we like, but they are not *bits* of it, identical with *c*'s, *e*'s, *g*'s and *c'*'s elsewhere. They simply resemble the *c*'s, *e*'s, *g*'s and *c'*'s elsewhere, and know these contents or objects representatively.

pressing them, make them self-contradictory (as the associationist psychology does when it calls them many ideas fused into one idea) or pretend to explain them (as the soul-theory so often does) by a barren verbal principle.

My intention was a good one, and a natural science infinitely more complete than the psychologies we now possess could be written without abandoning its terms. Like all authors, I have, therefore, been surprised that this child of my genius should not be more admired by others—should, in fact, have been generally either misunderstood or despised. But do not fear that on this occasion I am either going to defend or to re-explain the bantling. I am going to make things more harmonious by simply *giving it up*. I have become convinced since publishing that book that no conventional restrictions *can* keep metaphysical and so-called epistemological inquiries out of the psychology books. I see, moreover, better now than then that my proposal to designate mental states merely by their cognitive function leads to a somewhat strained way of talking of dreams and reveries, and to quite an unnatural way of talking of some emotional states. I am willing, consequently, henceforward that mental contents should be called complex, just as their objects are, and this even in psychology. Not because their parts are separable, as the parts of objects are; not because they have an eternal or quasi-eternal individual existence, like the parts of objects; for the various 'contents' of which they are parts are integers, existentially, and their parts only live as long as *they* live. Still, *in* them, we can call parts, parts.—But when, without circumlocution or disguise, I thus come over to your views, I insist that those of you who applaud me (if any such there be) should recognize the obligations which the new agreement imposes on yourselves. Not till you have dropped the old phrases, so absurd or so empty, of ideas 'self-compounding' or 'united by a spiritual principle;' not till you have in your turn succeeded in some such long inquiry into conditions as the one I have just failed in; not till you have laid bare more of the nature of that altogether unique kind of complexity in unity which mental states involve; not till then, I say, will psychology reach any real benefit from the conciliatory spirit of which I have done what I can to set an example.

CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF COLUMBIA COLLEGE. (III.)

Experiments on Dermal Sensations.[1]

BY HAROLD GRIFFING.

The Relation between the Intensity of the Stimulus and its Estimated Intensity.

Two stimuli differing greatly in intensity were successively applied to the hand of the observer, and he was required to judge how much greater one was than the other. The pressure was given by weights placed in the pan of a balance, and was transmitted to the hand by a wooden rod attached to the pan. The stimuli were 2, 10, 50, 250, 1250, and 1800 grams. The area of stimulation was that of a circle 4 mm. in diameter. The experiments made on four observers showed that on the average 10 g. was considered about twice as heavy as 2 g.; 50 g. twice as heavy as 10 g.; 250 g. three times as heavy as 50 g.; 1250 g. five times as heavy as 250 g.; and 1800 g. three times as heavy as 1250 g. It thus appears that for low and moderate intensities the estimate of intensity increases much more slowly than the objective intensity; but as the stimulus approaches the pain threshold, the reverse appears to be the case. Individuals differ, however, in their underestimation of low intensities, and also, but to a greater degree, in their overestimation of high intensities.

The Discrimination of Weights of Different Intensities.

Cylindrical boxes filled with shot served as stimuli. The method used was that of right and wrong cases; that is, the stimuli were placed successively upon the hand, and the ob-

[1] A full account and discussion of these experiments will be found in the writer's dissertation, *On Sensations from Pressure and Impact.* Supplement Monograph (No. 1) to this Review.

server was asked to decide which was heavier. The accuracy of discrimination is measured by the probable error, or that increment which the observer perceives correctly 75% of the time.[1] Thus the greater the probable error the less the accuracy of discrimination. The stimuli varied from 100 to 3200 g., no more than four intensities being used for any one observer. The results of 9040 experiments made on 5 observers showed that the probable error for pressure stimuli tends to increase in proportion to the intensity of the stimulus within the approximate limits 300–3000 g. For low intensities the probable error increases much more slowly than the stimulus. For 5–7 g. the probable error for a good observer was $\frac{2}{5}$ of the stimulus. For high intensities also there seems to be a similar tendency, but it is not so marked. As illustrative of our results, we give the probable errors in grams for McW.: for 100 g., 19; for 500 g., 36; for 1500 g., 112; for 3200 g., 193. The average value of the probable error for all stimuli (100 g. and above) and all observers was approximately $\frac{1}{9}$ of the stimulus. That is we can, on the average, judge correctly whether one stimulus is heavier or lighter than another 75% of the time when the stimuli are in the ratio 9 : 10.

In these experiments the constant error, or tendency to overestimate the second stimulus, was found to be for some persons very great, running as high as $\frac{1}{3}$ of the stimulus. The constant error is more variable than the probable error; the expression 'constant error' is thus quite misleading. The constant error seems to be greater for observers having a large probable error. A great constant error for pressure is not necessarily accompanied by a similar overestimation for lifted weights.

The degree of confidence was studied by having the observers say *a*, *b*, *c* and *d*, according as they were certain, quite confident, less confident, or doubtful. Individuals differ greatly in their confidence, the percentage of wrong judgments of which observers were confident varying from 2% to 33%. The probability of correctness when confident

[1] This quantity has been considered to be equivalent to the least noticeable difference. It is doubtful, however, if such a relation can be justified.

was for most observers about .8 to .9. There appears to be no relation between these quantities and the accuracy of discrimination. The percentage of correct guesses varied from 52% to 70%, the average being 59%.

The Place of Stimulation.

The accuracy of discrimination for weights of 100 g. or more is not for two observers appreciably different for the palm of the hand, the back of the hand, and the volar surface of the third phalanx of the index finger. For 5–7 g. it was found at first to be much less for the back of the hand and wrist than for the index finger of one observer, but to increase greatly by practice. Stimuli of low intensity, 5 and 100 g., when placed on the forearm, tended to be judged lighter than when placed on the finger. This result was obtained by placing a weight first on the finger and then on the arm, increments being added until the weights seemed equal.

The writer tested the sensitiveness to pain at different parts of the body by the algometer.[1] It was found that the sensitiveness is greatest where the skin is thin and not separated from the bone by other tissues. Among the most sensitive parts are the upper regions of the head, whereas the palm of the hand, the thigh and the heel are among the least sensitive parts.

Sensations from Impact.

The tactile threshold for pressure stimuli without movement was found by observing the angular elevation of a bristle which was attached at one end to a wooden handle, and at the other could transmit pressure to the skin. In this way it was found that .4 g. is about as easily perceived when movement is thus excluded, as is .01 g., when the stimulus is placed carefully upon the hand. The difference in the results is due to the sensory effect of movement.

By dropping weights upon the hand, the heights were found at which different weights caused pain. The weights were 25, 100, 200 and 300 g. The area of stimulation was

[1] An instrument by which pressure could be exerted up to 15 k.

constant, a circle about 1 cm. in diameter. The results of 60 measurements showed that the product of the mass and height pain-thresholds is fairly constant. As the height through which a body falls is proportional to the square of the velocity, the pain threshold and therefore the intensity of pain, depend as much upon the square of the velocity as upon the mass of a striking object.

By the method of right and wrong cases we studied the accuracy of discrimination for impact stimuli. The results of 800 experiments showed that a weight of 50 g., falling through 17.5 cm., is judged about as well as 1000 g. without movement. The average probable error for pressure only was $\frac{1}{11}$ of the stimulus for S. F., and $\frac{1}{14}$ for L. F. For impact the corresponding values were $\frac{1}{10}$ and $\frac{1}{12}$.

In 900 experiments, carried on in the same way, the weight was kept constant and the observer required to estimate differences in the intensity of the blow due to differences in height and therefore velocity. The results were compared with those based upon the same number of experiments on the same observers, in which the height was constant and the weight variable. We found that, on the whole, differences in weight are judged less accurately than differences in velocity, but more accurately than differences in the square of the velocity. But great individual variations occur.

Experiments were also made on the intensive effect of the weight as compared to that of the velocity. A 100 g. weight having fallen upon the hand from a height of 5 cm., the height was found at which 25 g. would cause a sensation of the same intensity. Here also observers differed greatly. The average height for 5 observers was 38 cm., the maximum being 58, the minimum 20 cm. Hence the mass has in general greater intensive effect than the height or the square of the velocity. Otherwise the average height found would be about 20 cm. On the other hand, the mass has less effect than the velocity or square root of the height.

The Area of Stimulation.

In the experiments on Weber's law two areas were used, 8 sq. cm. and .12 sq. cm. approximately. It was found that

on the whole this difference of area did not affect the accuracy of discrimination for weights. Individual variations, however, were very marked.

If stimuli of the same weight, but different areas, be placed successively upon the hand, the stimulus applied on the smaller area will be overestimated. By applying the method of right and wrong cases we measured this overestimation. The results of 400 experiments on one observer gave an overestimation of $\frac{1}{3}$ of the stimulus at 200 g. Experiments by a different application of the method of right and wrong cases on 5 observers gave about the same result, except that one observer showed a tendency to underestimate, rather than overestimate, the stimulus applied to the smaller area. By a third method, however, we found a decided overestimation for only 2 out of 5 observers. From these experiments on 10 observers, we conclude that this tendency is by no means universal.

The effect of alterations in the intensity of pressure on the accuracy of discrimination of areas was investigated by the method of right and wrong cases, differences in area being judged instead of differences in intensity. The standard areas used were 1 and 8 sq. cm. and the intensities 200 and 800 grams. The results of 1900 experiments on 3 observers showed that the accuracy of discrimination for areas was, on the average, about $\frac{1}{3}$ greater for 200 g. than for 800 g.

By placing thin circular cards upon the hand and applying pressure upon these, we studied the effect of variations in the area on the so-called tactile threshold. The areas were approximately 1 mm., 10 mm. and 90 mm. The averages of the corresponding threshold values, based upon 60 experiments, were for F., .2 g., .9 g. and 1.9 g.; and for the writer, .5 g., 1.4 g. and 1.6 g. Thus the smaller the area the greater the probability that stimuli of low intensity will be perceived.

In a similar manner the relation of the pain threshold to the area of stimulation was investigated. The average values of the pain threshold, based upon 80 experiments on two observers, were: for 10 mm., 1.4 kilog.; for 30 mm.,

2.8 kilog.; for 90 mm., 4.4 kilog.; and for 270 mm., 6.6 kilog. Thus the pain threshold increases with the area: but, like the tactile threshold, much more slowly than in direct proportion.

The Time of Stimulation.

The sensory effect of pressure stimuli of low intensity was found to depend upon the rate at which the pressure was increased. The instrument used was that referred to in the experiments already described on the tactile threshold. By this pressure was exerted upon the palm of the observer's hand up to .4 g., at different rates of increase. These rates were approximately .05 g., .3 g. and 2 g. per second. The corresponding percentages of times the stimulus was perceived in 300 experiments on 2 observers were 6%, 32%, and 82%. Thus the greater the rate of increase the greater the probability of perception.

The time in which dermal stimuli of different intensities cause pain was found in the following manner. Different weights were placed in a balance pan so as to press upon the palm of the hand, and the time was noted which elapsed before the appearance of pain. The pressure was communicated from the pan to the hand by a wooden rod fastened to the pan. The diameter of the base was 1.5 mm. The averages in seconds, based upon 80 experiments on 2 observers, are as follows: for 100 g., 230 sec.; for 200 g., 35 sec.; for 300 g., 10 sec.; for 500 g., 4.5 sec. It is evident, therefore, that the time as well as the area and intensity of stimulation determine the sensory effect. There is, however, an intensive limit, below which pressure stimuli never become painful. This is probably from 25 to 50 g. for the area used.

The After-Image Threshold.

BY SHEPHERD IVORY FRANZ.

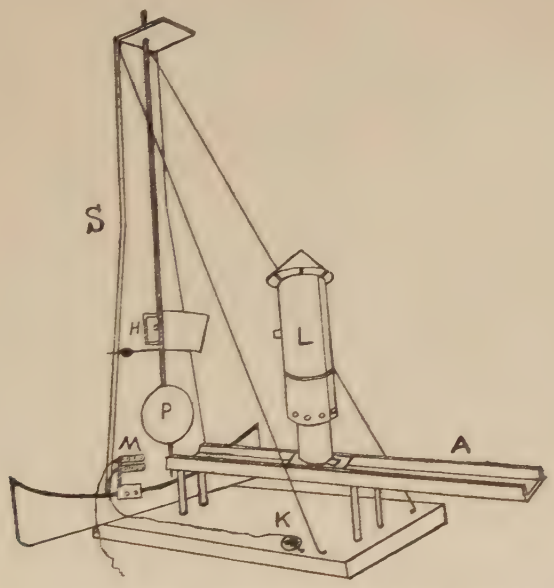
Ever since Aristotle described in his De Somniis[1] the appearance of an after-image, the phenomena have attracted

[1] This seems not to be generally known by German writers. Aubert and Helmholtz both credit Peiresc as being the first to mention after-images.

attention. St. Augustine mentions them, and in modern times such prominent men as Buffon, Goethe and Newton have described their appearance. But very little was accomplished beyond the making of theories until this century, when Plateau, Seguin, Fechner and others studied the color changes. Up to the present time practically nothing has been accomplished in the way of exact measurement.

The present paper gives the results of an attempt to measure the smallest amount of light which will produce an after-image. For this purpose three physical units had to be considered—the intensity of the light, its area, and the time of stimulation. The apparatus used was planned and formerly used by Prof. Cattell, but was adapted by the writer. It is represented in the accompanying cut.

FIGURE 1.



S is an upright iron screen pierced by a hole (H) through which the light from the hooded lamp (L) may pass to the observer on the other side of the screen. P is a seconds pendulum. To this is attached a piece of sheet iron which covers the hole when the pendulum is held up by the electro-magnet (M). The key (K) which makes and breaks the current to the magnet (M) is managed by the experimenter, and

the pendulum is held up or let swing at his pleasure. By breaking and making the current the pendulum swings, permits the light to be seen by the observer for exactly one second, and is caught up again by the magnet. The lamp is moved along the arm (A), increasing or decreasing the intensity of the light. The opening (H) was covered with ground glass. $\frac{1}{100}$ candle power was found a convenient intensity, this being increased by moving the lamp nearer the observer, and decreased by moving it away from the observer. The lamp was used at the distances $\frac{1}{4}$, $\frac{1}{2}$, 1, 2 and 4 meters, and so far as the intensity decreases inversely as the square of the distance, the respective intensities would be $\frac{4}{25}$, $\frac{1}{25}$, $\frac{1}{100}$, $\frac{1}{400}$ and $\frac{1}{1600}$ candle power. The absorbing power of the ground glass was found to be 50%, whence the intensities were decreased by half—making the series—$\frac{2}{25}$, $\frac{1}{50}$, $\frac{1}{200}$, $\frac{1}{800}$, $\frac{1}{3200}$, c. p. In the experiments on intensity, the time of exposure (one second) and the area (64 sq. mm.) were kept constant. For the experiments on area, the lamp was placed at a distance of $\frac{1}{4}$ m., thus making the intensity $\frac{2}{25}$ c. p., the time (one second) being the other constant. The area was changed by using different pieces of ground glass on which black paper blocked off all but the small area required. The areas used were 64, 16, 4, 1, $\frac{1}{4}$, $\frac{1}{16}$ sq. mm. When time was the changeable unit, the area (64 sq. mm.) and the intensity ($\frac{2}{25}$ c. p.) were the constants. The series consisted of four times, $\frac{1}{1000}$, $\frac{1}{100}$, $\frac{1}{10}$ and 1 second. The shorter times were obtained by means of drop screens, made of pasteboard and weighted. As they did not fall in grooves there was no appreciable friction, and hence the real time practically corresponded with the theoretical time. The screen was on the side of the apparatus near the observer, and therefore is not shown in the cut. The time one second was given by the pendulum. As will be noticed, there was a common unit in the three series, *i. e.*, when the experiments were made with 1 sec., 64 sq. mm. and $\frac{2}{25}$ c. p.

The experiments were conducted in a dark room, and all observations were made with the eyes open, so as not to disturb the after-image. A cloth curtain was hung across the room, shutting off from the observer everything but the

small opening in the screen. The observer's eyes were 30 cm. from the opening, his head being steadied by a support. Before any experiments were made a rest of ten minutes was taken to allow the observer's eyes to become accustomed to the darkness; between the disappearance of one after-image and the next stimulus there was a rest of thirty seconds. When the thirty seconds had elapsed a signal was given, five seconds were allowed for preparation, and the stimulus was produced.

Very few difficulties presented themselves, and of these the only one not overcome was the lack of a fixation point, as any fixation point was apt to produce a disturbing after-image. By practice, however, the observer learned to look in a certain way for the stimulus, and in the case of the writer not over five per cent. of the time were the eyes consciously focussed after any part of the light was seen. The kerosene lamp used was trimmed at the beginning of the experiments. By photometric determinations always made before a sitting and generally during and after the sitting, it was found that the light varied very little or not at all.

Four observers were tested, C., McW. and S. respectively with time, area and intensity. All were advanced students in psychology, and S. had had previous experience with after-images. F., the writer, was the fourth observer, the three series being made upon him.

The results of nearly 3,000 experiments are given in the following tables. In the first line the percentage of times an after-image was seen is given, and in the second line the average variation of the sets of ten trials; 100 experiments of each sort were made, excepting in those cases in which a different number is given in parenthesis.

Some preliminary experiments on area made on the writer bear out in general the results in the corresponding series. These experiments were made with an intensity of $\frac{1}{20}$ c. p., so that they could not be combined with the others. The other constant was an exposure of one second. The same areas were used except that the $\frac{1}{16}$ sq. mm. was omitted. Seventy experiments were made on each area. The results, with the average variations, are shown in the accompanying table.

INTENSITY.

Intensity in candle power.		$\frac{2}{25}$	$\frac{1}{50}$	$\frac{1}{200}$	$\frac{1}{800}$	$\frac{1}{3200}$
S.	Percentage,	100	94	48	17	2
	Variation,	— (80)	7	25 (110)	20.8 (110)	3.6
F.	Per cent.,	100	96	44	15.5	1
	Var.,	—	5.4	19.5 (130)	13.5 (130)	1.8

AREA.

Area in square mm.		64	16	4	1	$\frac{1}{4}$	$\frac{1}{16}$
McW.	Per cent.,	100	90	72	52	27	20
	Var.,	— (50)	7.5 (80)	14.8	12.4	9.2	6
F.	Per cent.,	100	96	88	57	31	8
	Var.,	—	4.8	8.8	13	15.4	8

TIME.

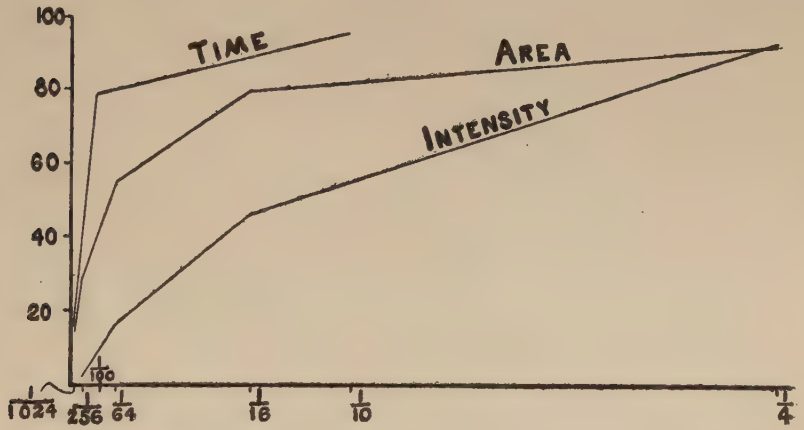
Time in seconds.		1	$\frac{1}{10}$	$\frac{1}{100}$	$\frac{1}{1000}$
C.	Per cent.,	97	95	75	12
	Var.,	3.5 (70)	7	17	10.8
F.	Per cent.,	100	97	82.5	19
	Var.,	—	4.2	8.3 (120)	7.4

AREA.

Area in sq. mm.		64	16	4	1	$\frac{1}{4}$
F.	Per cent.,	96	89	67	41	19
	Var.,	5	10	14.7	10	9.9

The results of the first three tables are represented graphically by the accompanying curves.

FIGURE 2.



The abscissa denotes respectively divisions of time, area and intensity, the ordinate the percentage of times an after-image appeared. The curves are not carried out to represent the greatest intensity, the greatest area and the greatest time. Each curve is the average of the two observers in that series, the close agreement of the observers making this method permissible. The figures on the abscissa represent the proportion of that stimulus to the greatest stimulus, taking respectively time, area and intensity as the variables.

If we regard the threshold as that intensity, time or area, which produces an after-image 75% of the number of stimuli, we conclude

(1). That with an exposure of one second and an intensity of $\frac{2}{25}$ c. p., the threshold is 4 sq. mm.

(2). That with the area 64 sq. mm. and the intensity $\frac{2}{25}$ c. p., the threshold is $\frac{1}{100}$ second.

(3). That with the area 64 sq. mm. and the time of exposure one second, the threshold is $\frac{1}{100}$ candle power (approximately), or between $\frac{1}{50}$ and $\frac{1}{200}$ c. p.

If we substitute in our definition 25%, or 50%, or 90%, for the 75%, we but change the figures to suit the case.

It is worth noting that of the 1,500 cases when after-images were seen, but five were negative, a proof of the theory that the negative a ter-image is due to exhaustion of the eyes, the low intensities, the small areas and the short times not being sufficient to tire or exhaust the eyes. These five negative images were all seen toward the close of a sitting, when the eyes had been used for forty or fifty experiments, and all were with the greatest intensity, the longest time and the largest area.

With the results obtained we are able to make a further comparison—a correlation of our physical units in terms of the production of after-images—a purely psychological problem. How much time equals how much intensity or area? A glance at the curves and percentages shows that equal increments in area, intensity and time do not give equal results. If we represent our constants by the letters c, c′ and c″ respectively for intensity, time and area, and let i, t and a represent respectively $\frac{1}{800}$ c. p., $\frac{1}{1000}$ sec. and $\frac{1}{16}$ sq. mm., from the table of percentages we get the following approximate equations.—

$$i\,c = t\,c' = a\,c''$$
$$(2\,i\,c) = (1.7\,t\,c') = (4\,a\,c'')$$
$$4\,i\,c = 3.2\,t\,c' = 16\,a\,c''$$
$$8\,i\,c = 10\,t\,c' = 64\,a\,c''$$
$$16\,i\,c = 100\,t\,c' = 256\,a\,c''$$

The 8 i c and the 3.2 t c′ represent $\frac{1}{100}$ c. p. and $\frac{3.2}{1000}$ sec. (approximately). These figures and the second equation in brackets are supplied from the curves. The relations, then, may be stated as follows: "Squaring the time equals doubling the intensity or quadrupling the area," and vice versa, "reducing the area to one-fourth equals halving the intensity and taking the square root of the time." Whether this be a chance relation or a general one throughout the phenomena of after-images cannot be dogmatically stated now. The writer has in view the further study of this problem.

THE NORMAL DEFECT OF VISION IN THE FOVEA.

BY CHRISTINE LADD FRANKLIN.

When the fact that the retina contains a substance which is chemically acted upon by light was first announced, it seemed that the secret of the transformation of energy of wave-motion into something capable of being transmitted along the nerve fibres and affecting the conscious organism as the sensation of light had been definitely, at least in its rough stages, unravelled. But immediately difficulties appeared: the substance could not be detected in the cones, and it was therefore apparently wanting in the fovea, the spot of most acute vision; and, moreover, certain classes of animals had retinas which contained none of the substance. It was therefore certain that the visual purple was not essential to vision, and the intense interest which it had at first aroused fell wholly into abeyance.

Prof. Ebbinghaus has recently returned to the subject, and has proposed to account for the apparent colorlessness of the cones by assuming in them a second substance of such a color as always to mark the presence of the visual purple. The visual purple (or visual blue, as it must be considered for this purpose, although its real color is only a very slightly bluish-red) and its product, the visual yellow, are the source of the sensations of yellow and blue respectively; the imaginary substance is, in its two stages, the source of the sensations red and green, and is for that purpose first green and then red in color. Now, a green and a purple substance, when present together, might, it is true, produce a colorless mixture, since purple and green are complementary colors; but a moment later these two substances have become respectively yellow and red. What

becomes of the complementariness then?—or when one is green and the other yellow?—or when one is red and the other purple? Or must we suppose that, although thousands of eyes have been examined, first and last, after every possible degree of exposure to light, and to color, still chance has brought it about that no stage of this series of processes has ever been lighted upon except the first? So short-sighted a theory as this,—one in which we must so carefully refrain from going beyond the first step of the imagined process,—has probably never before been seriously proposed for acceptance.

But the suggestion of Prof. Ebbinghaus has had this good effect, that it has induced Prof. König to undertake an accurate determination of the relative absorption of the visual purple for different kinds of homogeneous light.[1] He proposed the question as a subject of investigation to Dr. Abelsdorff and Frl. Köttgen. A spectro-photometer especially designed for the purpose was constructed, and it was hoped that the skill and experience gained in the study of the visual purple of the frog they might, in course of time, be able to apply to a human retina, if good luck should throw one in their way. But, as it happened, the apparatus was no sooner set up in one of the dark rooms of the laboratory than they received word that a human retina was to be at their disposal; and Dr. Abelsdorff being suddenly called away, the study of it was carried out by Prof. König and Frl. Köttgen. The patient to whom the eye belonged remained in absolute darkness for twenty hours before the operation. The eye was extracted by the light of a sodium flame, put at once into an intensely black box, and rapidly conveyed to Prof. König's laboratory. Here it was opened, twenty minutes after leaving the living body, with all the necessary precautions, by an oculist who had already made himself familiar, by means of the ophthalmoscope, with the exact position of the melano-sarcoma which had caused the eye to be extracted. The entire retina, with the exception

[1] *Ueber den menschlichen Sehpurpur und seine Bedeutung für das Sehen.* Nach gemeinschaftlich mit Frl. Else Köttgen ausgeführten Versuchen. Sitzungsber. d. Akad. d. Wissensch. zu Berlin, 21 Juni, 1894.

of the diseased portion, was put into a solution of gallic acid, and after filtration a sufficient amount of the extract was obtained to fill twice the minute absorption-box of the spectrophotometer. With the first filling the absorption of the visual purple was obtained and compared with the absorption of the (not absolutely clear) solution which remained after the purple (crimson) color had been wholly bleached out; the second filling sufficed for a redetermination of the absorption of the visual purple, and for that of the visual yellow, which was obtained after the purple had been bleached for that color. (The two determinations of the absorption of the purple substance are in close agreement with each other.)

It was at once evident that the absorption distribution in the spectrum of the purple substance coincided roughly with the spectral distribution of brightness for the congenitally totally color-blind, and also with the spectral distribution of brightness for the normal eye (as well as for the partially color-blind) at a very faint degree of luminosity. The suggestion was a natural one that it is the vision of the totally color-blind, and of the normal eye in a faint light, which is dependent upon the absorption of light by the visual purple. The curves of sensation in these two cases were reduced to a spectrum of equal distribution of energy by means of Prof. Langley's determination of the distribution of energy throughout the spectrum. Correction was also made for the absorption of the macula lutea and for that of the crystalline lens (freshly determined for an individual of the proper age). It then became evident that the coincidence between the three curves is remarkably close. (That the two curves of sensation referred to are in close agreement with each other had, of course, already been shown by Hering.) It was evident, that is, that the absorption in the purple substance is very exactly proportional to the value of light as an exciter of sensation (1) in the totally color-blind, and (2) in all other eyes at an intensity so faint that colors are no longer visible. Prof. König had also convinced himself of the existence of a similar coincidence between the absorption of the visual yellow and the blue constituent of

the colors of the spectrum as already determined by himself and Dieterici (*Zeitsch. f. Psych. u. Phys. der Sinnesorgane*, IV., S. 241).

But the difficulty still remained which had originally caused the visual purple to fall into neglect,—the substance is apparently wanting in the cones, and therefore in the fovea. To meet this difficulty two assumptions were possible: either, that the cones do contain the purple substance, but in so decomposable a form that it can never be detected objectively, no matter what the precaution used in extracting the eye; or, that the eye is actually blind in the fovea in the two cases in question. In favor of the first assumption was the fact that, if the yellow substance is really the source of the sensation of blue, then it must be supposed to exist in a *less* decomposable state in the periphery of the eye to account for the fact that we are then nearly blind to blue;[1] it therefore 'lies near' to assume (when some assumption is absolutely necessary) that it exists in a *much more* decomposable state in the fovea, and that it has for this reason hitherto escaped detection. But I was most anxious to put the second of these assumptions to the test,—the more so as I had already made the prediction that the cause of total color-blindness is a defective development of the cones;[2] and also that the function of the visual purple is to render possible that form of vision which does not exist until after a delay of twenty minutes or so in a dark room;[3] both predictions being naturally suggested by my theory of light-sensation. I had also pointed out, in the last-mentioned paper, that the visual purple cannot exist in the cones, even in a bleached-out state, because the visual purple is fluorescent, and the more so the more it is bleached out, while the

[1] Gad, in his criticism of the papers of König and Zumft, about to be mentioned, implies (p. 499) that Prof. König found the blue-blindness of the fovea forced upon him by his hypothesis regarding the function of the visual purple and of the visual yellow. That was not the case; Prof. König had adopted the first of the two assumptions here affirmed to be possible, and it was only some six weeks later that the defective vision of the fovea was discovered.

[2] *Zeitsch. f. Psych. u. Phys. der Sinnesorgane.* Bd. IV. s. 9.

[3] *Professor Ebbinghaus' Theory of Colour Vision.* MIND, N. S. Vol. III, p. 103.

fovea remains as a dark spot in the ultra-violet rays of the spectrum; and the more strikingly dark the more the rods in the neighborhood have become fluorescent (p. 100). On the other hand, Prof. König pointed out to me that even if vision should be wholly wanting in the fovea of a totally color-blind individual, it would hardly be possible to detect it, for he would unquestionably have acquired the habit by avoiding the use of this spot. This suggestion was, therefore, not immediately carried out. But it was arranged that I should take for the subject of my investigation for the summer a re-determination of the threshold of sensation for different parts of the retina and for different kinds of monochromatic light. A plan of work was built up in two of the dark rooms of the laboratory, and I have to express my gratitude to Prof. König for his untiring patience in assisting me to overcome the difficulties which one after another presented themselves.[1] The preliminary observations for eliminating the sources of error consumed some time, and I then made a first determination of the variation in the intensity of light necessary in order to be just perceptible, or of its inversion—the sensitiveness of the eye to faint impressions—at different distances from the fovea. I even drew the curves, and found them to present a maximum at a distance of about 25°, at which point the sensitiveness of the eye is about four times as great as at the fovea, while at a distance of 50° the sensitiveness is still about twice as great as at the fovea. E. Fick found the maximum to be at about 15°, but that was without making correction for the diminished area of the pupil of the eye when light enters it very much from the side. The shape of the curves is not noticeably different for different parts of the spectrum. These curves are a representation of the diminished sensitiveness in the region of the fovea, which has long been known, and which has been especially forced upon the attention of astronomers when looking for faint stars with the naked eye. I had been in the end for several weeks at work in my dark room for the express purpose of finding that the fovea is *blind* to impressions so faint as those with which I was occupied, before

[1] The full results of this investigation will be published later.

I found it; although, after it has once been seen, it seems incredible that it can ever have been overlooked. It finally dawned upon me—not that the bright point directly looked at was invisible—but that by giving what I can only describe as a certain curious twist to the eye, a certain bright point could be caused to disappear.[1]

The reason that the 'normal night-blindness of the fovea,' as this insensitiveness to the faint-light sensation may best be called, has been completely overlooked by all other observers, and also by E. Fick and by Kirschmann, who have made a special investigation of the threshold of sensation for different parts of the retina, is very plain: the unconscious ego, which takes so large a part in regulating the action of even the voluntary muscles, is well aware of this blindness, and takes pains that an image of a small object shall almost never fall upon this spot. In a faint light, *to look at*, which is usually a phrase of two-fold significance, meaning, namely, to turn the eye in such a way that its power of seeing is a maximum; and also to turn the eye so that the image of the object looked at falls on the fovea, has now the two elements of its significance disjoined; when vision is at a maximum (or when it is possible at all), it is necessary that the image should fall a little to one side of the fovea, and that is the motion with which the subjective feeling of fixation is associated. Not only did the faint object which I was engaged in observing disappear, but also the two (much brighter) spots of phosphorescent paste (which are used in order to secure a fixation-point halfway between them) could be made to completely vanish by 'looking at' them, in the new sense of that phrase. This phosphorescent matter gives a spectrum which is almost wholly blue.

Having convinced myself of the existence of this faint-light foveal blindness, it was necessary to devise a method by which the total blindness of the fovea of the totally color-

[1] This motion of the eye can be facilitated if one brings in the aid of a strong desire not to see the point. This would seem to show that the knowledge of the existence of this blind spot, while almost wholly below the level of consciousness, is yet not altogether withdrawn from an interaction with the conscious content of the organism.

blind patient, who was soon to return to Prof. König's laboratory, could be demonstrated. It was not permitted to subject his eyes to any strain, and it was not probable that a rather feeble boy of thirteen could easily learn to execute a motion which had hitherto been absolutely avoided, not only by him but by all the rest of the world; and which, besides, there was no possibility of describing to him. But it naturally suggested itself to me very soon that it would only be necessary to give him a group of closely contiguous isolated bright points to look at, and that chance would see to it that one or the other of them should now and then fall into the dark hole of his fovea. The same device has proved effective for exhibiting the faint-light blindness to a person who has not yet learned to execute the motion of the eye necessary to cause a single spot to disappear. Prof. König at once made use of this method to show that even the most intense blue that could be thrown into the field of his spectro-photometer, by the light of the oxyhydrogen blow-pipe, is insufficient to cause any sensation whatever in the fovea. No difficulty was experienced in demonstrating the total blindness of the totally color-blind boy in this spot, although it was quite impossible to get him to experience the invisibility of a single bright point when only one was in the field. This individual had a definite spot at one side of the fovea, which he constantly made use of as a fixation-spot; the nystagmus, which is a common accompaniment of total color-blindness, is readily explained as the expression of there being no such favored substitution fovea. The remarkable diminution of visual acuity on the part of such patients, which has not hitherto been understood, is seen to be very natural when it is known that their fovea is not in a condition to perform its function. Prof. König proceeded at once to make a series of color-equations in the fovea—a work of extreme difficulty—from which it appears that the condition, which extends over an area of from 55′ to 70′, is that of a typical blue-blindness.

To the facts already described, Prof. König adds a contribution recently made by himself and Dr. Zumft,[1] by which

[1] *Ueber die lichtempfindliche Schicht in der Netzhaut des Menschlichen Auges.* Sitzungsberichte d. Akad. d. Wissench. zu Berlin, 24 Mai, 1894.

they would seem to have shown that light of different colors is perceived in different layers of the retina, and blue distinctly in front of green, yellow and red. The method consists in throwing two shadows of a blood-vessel upon the back of the retina, by means of two holes in a card, which is constantly moved to and fro in the front focal plane of the eye. The distance apart of the two shadows they were able to measure, and they found it to be different for differently colored homogeneous light; and the calculated distance of the blood-vessel from the layer of retina which is affected by the light, they found to be, for several portions of the spectrum examined:

λ 670	0.44 mm.
590	0.44
535	0.41
486	0.38
434	0.36
White, ...	0.41

Prof. König interprets this to mean that the space between the layer in which blue is perceived, and that in which red is perceived, is *greater* than the thickness of the end members of the rods and cones, and hence that one must infer that the pigment epithelium also is a layer sensitive to light. It would seem, however, that there must be something about these experiments the meaning of which is not yet wholly cleared up, for the length of the outer member of a rod is only .025 to .03 mm., and that of an epithelium cell is only about half as much again. They do not, therefore, together form a layer of sufficient thickness to take in the difference of .08 mm., which the observations require. The experiment, therefore, proves too much. Again, Prof. König's interpretation of the facts here enumerated, as meaning that the visual yellow is the source of the sensation of blue; that green, yellow and red are all perceived in the pigment-epithelium, and that the *cones* are merely lenses for concentrating light upon the epithelium cells, makes no provision for the nerve-conduction of any effect of light in the epithelium. In the fovea there would be absolutely no means of such conduction except by way of the cones, and

it is difficult to conceive that organs which are performing the part of lenses should also be able to function as conductors.[1] Again, the recent brilliant work of Ramon y Cayal and others on the minute anatomy of the retina discloses such close similarity (together with a perfectly definite difference) between the rods and the cones, as regards structure and connections, as to make it very unnatural to assign to them functions of a widely different nature. Prof. König says (p. 4) that the results here communicated "are in contradiction (1) with the theories of Hering and Ebbinghaus, according to which a single substance forms the basis of the red and green sensations on the one hand, and of the blue and yellow sensations on the other hand; and (2) with the theories of Donders, Wundt and Franklin, according to which all colors are perceived in a single substance." It is true that all these theories would be rather hard hit by these results, if the results themselves were not involved in some obscurity. As it is, however, it may perhaps be safe to wait until the discrepancies pointed out have been, to some extent at least, cleared up.

There is yet one more recent contribution from König's laboratory which has an important bearing upon the new facts already mentioned. Brodhun, and more recently Tonn, have shown that the Purkinje phenomenon consists in a change in the blue constituent of white light—the red and green remaining unchanged; this would seem to indicate that the increased amount of coloring matter in the rods, as the intensity of light begins to diminish, furnishes a means for an increased amount of absorption, and would seem to point, it must be confessed, to the rods as the seat, at least in part, of the sensation of blue.

Farther elements of the theory of light-sensation now advocated by Prof. König are these:

[1] Prof. Gad (whose paper has reached me since writing the above) makes the farther criticism that only the first surface of the pigment-cells would be available, because light cannot pass through even a very thin layer of the fuscine which gives them their dark color. But he apparently forgets that, under an ordinary degree of illumination, the pigment grains are nearly all heaped up *between* the visual elements, and that the body of the pigment cell is left almost free from them. (*Der Energieumsatz in der Retina.* Separat-Abzug aus Arch. f. Anat. u. Phys., 1894.)

1. The visual purple is the photo-chemical substance whose decomposition causes the faint light sensation. That sensation is in reality blue, although we are not aware of it.

2. The visual yellow is the source of the sensation of blue at ordinary intensities.

3. The white, and also all shades of grey, of an ordinary illumination, are of a very different origin from (a) the sensation of grey in a faint light, (b) the sensation of the totally color-blind, (c) the sensation of the normal eye in the periphery; they are (as in the original Young–Helmholtz theory) a synthesis in 'judgment' of the sensations red, green and blue.

As regards Prof. König's interpretation of the new facts, the following observations remain to be made:

(*a*). There is no occasion for assuming that the visual purple is, by its decomposition, the source of the sensation. All that is forced upon us is that absorption by the visual purple acts as a means of *re-inforcement* at a time when light would be too feeble to perform its function without the presence of a special agent for absorbing it. That the visual purple and the visual yellow should, by their decomposition, furnish the same sensation (blue) is very hard to believe, in view of the fact that the visual yellow is, beyond all question, itself one of the decomposition products of the visual purple, and that their decomposition products can therefore not possibly be the same.

(*b*). Becker's case of congenital monocular total color-blindness, many cases of acquired monocular total color-blindness, and the consciousness of every individual in a faint light, all speak against the hypothesis that blue, and not grey, is perceived under those circumstances; still more, the perfect conviction which one has that a bit of colored paper whose image is removed to the periphery of the eye fades into a grey which is indistinguishable from the grey of direct vision.

(*c*). There is no doubt whatever that the eye has a perfectly unimpaired vision for the whole length of the spectrum when the light is so strong that the rod-yellow has been completely bleached out. That can, therefore, not be the

photo-chemical substance for blue. The eyes of the totally color-blind undergo adaptation.[1] The rod-purple in their eyes, therefore, suffers changes in its quantity exactly as we should expect it to do from what we know of the substance elsewhere. There is, therefore, every reason to believe that it is, like all rod-purple which we have ever examined objectively, completely bleached out in a bright light, and hence that it is not the sensation-producing substance, but merely a means of re-inforcement for waning light.

(*d*). The fact that the adaptation-substance is purple in color serves a useful purpose. The most common faint light of nature is the faint light of dense forests, which is green. The rod-pigment is therefore especially adapted to the absorption of the only light which penetrates them. How completely the light at the bottom of forest trees has been sifted of the light which their leaves absorb has been shown quite recently by an investigation into the growth (or rather non-growth) of nearly all ground plants after the foliage has fully come out in the late spring.[2]

(*e*). Almost the only function of the extreme periphery of the eye is the *detection of motion*,—that is, the detection of changes in the distribution of light and shade. The changes in the rod-pigment bring about a constant complete adaptation to the *existing* pattern of light and shade,—build up a counter-pattern, so to speak, upon the surface of the retina,—and only a *new* distribution of light (*i. e.*, the entrance of an enemy upon the field) causes any sensation. This function of the periphery is facilitated by the fact, made out by Ramon y Cayal, that there are numerous large, horizontal connecting cells which must play the part of re-inforcing a sensation by spreading it over a wide area, at the same time that they diminish the sharpness of its localization; the indistinctness of vision in the periphery has

[1] Just before leaving Berlin in September I made a journey to the place where the color-blind boy above referred to was spending the summer, in order to determine this point. Hering mentions that his case could see better in a dark room than those having normal eyes, but he does not say whether his vision improved with time. (*Untersuchung eines total Farbenblinden.* Pfl. Arch. Bd. 54 S. 10.)

[2] *Klebs: Einfluss des Lichtes auf die Fortpflanzung der Gewächse.* Biol. Centralbl. XIII, 641.

long been known to be much greater than the indistinctness of the image formed there would account for.

(*f*). If the rod-pigment, in both of its stages, is merely a reinforcement agent, then all theories of light-sensation (except, indeed, that of Ebbinghaus, which loses whatever plausibleness it may be supposed to have had) may be considered to remain very much in the same condition in which they were before.

IV. PROCEEDINGS OF THE THIRD ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

PRINCETON UNIVERSITY, PRINCETON, N. J., 1894.

REPORT OF THE SECRETARY AND TREASURER FOR 1894:

The third annual meeting of the American Psychological Association was held at Princeton College, Princeton, N. J., on Dec. 27 and 28, 1894. Prof. William James, President of the Association, presided over the sessions, which lasted from 10.30 A. M. on Dec. 27 to 4.30 P. M. on Dec. 28. President Patton, of Princeton College, made an address of welcome on Thursday afternoon, and entertained the members of the Association in the evening, after the address of the President of the Association. Abstracts of the papers read at the meeting are subjoined. Papers by Prof. Starr and Prof. Hume were presented in the absence of their authors, and papers offered by Prof. Jastrow, Prof. Delabarre, Prof. Titchener, Mr. Pierce and Dr. Witmer were not read.

The members in attendance were: Alexander, Baldwin, Cattell, Chrysostom, Farrand, Hyslop, Franklin, James, Ladd, MacDonald, Marshall, Mead, Mezes, Mills, Miller, Newbold, Ormond, Pace, Royce, Sanford, Strong, Warren —twenty-two in all. In addition, the sessions were well attended by professors and advanced students from the different universities and colleges.

The following nominations for membership were made by the council, and the elections were made by the Association:

Prof. Archibald Alexander,	New York.
Dr. John Bigham,	University of Michigan.
Prof. Charles L. Dana,	Bellevue Medical College.

Mr. E. A. Kirkpatrick,	Winona, Minn.
Dr. A. Kirschmann,	University of Toronto.
Prof. S. E. Mezes,	University of Texas.
Mr. W. T. Shaw,	Wesleyan University.
Prof. James Seth,	Brown University.
Prof. Paul Shorey,	University of Chicago.
Prof. H. M. Stanley,	Lake Forest University.
Miss M. Washburn,	Wells College.

A constitution was adopted, as follows:

CONSTITUTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

ART. I. *Object.*—The object of the Association is the advancement of Psychology as a science. Those are eligible for membership who are engaged in this work.

ART. II. *The Council.*—A Council shall be elected from the members of the Association as an executive. The Council shall consist of six members, two being elected annually for a term of three years. The President shall be *ex-officio* a member of the Council. The Council shall nominate officers for the Association, shall nominate new members, and shall make other recommendations concerning the conduct of the Association. The resolutions of the Council shall be brought before the Association and decided by a majority vote.

ART. III. *Officers.*—There shall be annually nominated by the Council and elected by the Association a President, a Secretary, and a Treasurer, who shall perform the usual duties of these officers.

ART. IV. *Annual Subscription.*—The annual subscription shall be $3, in advance. Non-payment of dues for two consecutive years shall be considered as equivalent to resignation from the Association.

ART. V. *Executive Committee.*—The President, the Secretary, and a member from the place where the meeting is held, shall be a committee to make necessary arrangements for the annual meeting.

ART. VI. *Proceedings.*—Such proceedings shall be printed by the Secretary as the Association may direct.

ART. VII. *Amendments.*—Amendments to the Constitution must be adopted by a majority vote at two consecutive annual meetings.

As prescribed by the Constitution, a Council was elected as follows:

Term expiring 1897:

Prof. G. T. Ladd, Yale University.
Prof. J. McKeen Cattell, Columbia College.

Term expiring 1896:

Prof. J. Mark Baldwin, Princeton College.
Prof. William James, Harvard University.

Term expiring 1895:

Prof. John Dewey, University of Chicago.
Prof. G. S. Fullerton, University of Pennsylvania.

Prof. J. McKeen Cattell was elected President and Prof. E. C. Sanford Secretary and Treasurer for the coming year.

An invitation was received from the American Society of Naturalists, inviting the Association to affiliate with it. The question was referred to the Council, with power to act. Invitations were received for the meeting of 1895 from Harvard University and the University of Chicago. The decision as to place of meeting was left with the Council, with the recommendation that the convention meet, if possible, at the same time and place as the Society of Naturalists. It was resolved that the minutes should be printed in such journals as were prepared to print them in full.

The report of the Treasurer is as follows:

Receipts:

Balance on hand, ...	$69.50	
2 dues, 1893, ...	6.00	
38 dues, 1894, ...	114.00	
Sales of Proceedings, ...	1.60	
		$191.10

Expenditures:

Printing Proceedings for 1893, as per Messrs. Macmillan & Co.'s voucher,	$55.93	
Postage, expressage and stationery, .	8.00	
		$63.93
Balance on hand, ...		$127.17

The account was audited by the Council and approved.

J. McKeen Cattell,
Secretary, 1894.

Abstracts of Papers.

(1.) *The Knowing of Things Together.* Address by the President, Prof. William James, Harvard University.

The synthetic unity of consciousness is one of the great dividing questions in the philosophy of mind. We know things singly through as many distinct mental states. But on another occasion we may know the same things together through one state. The problem is as to the relation of the previous many states to the later one state. It will not do to make the mere statement of this problem incidentally involve a particular solution, as we should if we formulated the fact to be explained as *the combination of many states of mind into one.* The fact presents itself, in the first instance, *as the knowing of many things together*, and it is in those terms that the solution must be approached.

In the first place, *what is knowing?* 1. *Conceptual* knowing is an external relation between a state of mind and remote objects. If the state of mind, through a context of associates which the world supplies, leads to the objects smoothly and terminates there, we say it knows them. 2. *Intuitive* knowing is the identity of what, taken in one world-context, we call mental content and in another object. In neither 1 nor 2 is there involved any mysterious self-transcendency or presence in absence. 3. This mystery does, however, seem involved in *the relation between the parts of a mental content itself.* In the minimum real state of consciousness, that of the *passing moment*, past and present are known

at once. In desire, memory, etc., earlier and later elements are directly felt to *call for* or *fulfil* each other, and without this sense of mutuality in their parts, such states do not exist. Here is presence in absence; here knowing together; here the original prototype of what we *mean* by knowledge. This ultimate synthetic nature of the smallest real phenomenon of consciousness can neither be explained nor circumvented.

We can only trace the particular conditions by which particular contents come thus to figure with all their parts at once in consciousness. Several attempts were then briefly passed in review. Mere synchronical sense-impression is not a sufficient condition. An additional inner *event* is required. The event has been described: *physiologically* as 1) 'attention;' as 2) ideational processes added to the sensorial processes, the latter giving unity, the former manyness; as 3) motor synergy of processes; *psychologically* as 4) the thinking of relations between the parts of the content-object; as 5) the relating of each part to the self; *spiritually* as 6) an act of the soul; *transcendentally* as 7) the diminution (by unknown causes, possibly physiological) of the obstruction or limitation which the organism imposes on the natural knowing-of-all-things-together by an Absolute Mind. For transcendentalism the problem is, 'How are things known separately at all?'

The speaker dealt with these opinions critically, not espousing either one himself. He concluded by abandoning the attempt made in his Principles of Psychology to formulate mental states as integers, and to refer all plurality to the objects known by them. Practically, the metaphysical view cannot be excluded from psychology-books. 'Contents' have parts, because in intuitive knowledge contents and objects are identical; and Psychology, even as a 'natural science,' will find it easier to solve her problem of tracing the conditions that determine what objects shall be known together, by speaking of 'contents' as complex unities.

[The address is printed in full in the *Psychological Review* for March, 1895.]

(2.) *Minor Studies, and Notes on New Apparatus.* By Dr. E. C. SANFORD, Clark University.

The four papers reported were on the following topics: (1) Comparative Observations on the Indirect Color Range of Children, Adults, and Adults Trained in Color, by Geo. W. A. Luckey. (This study was made in the Psychological Laboratory of the Leland Stanford, Jr., University); (2) A Study of Individual Psychology, by Miss Caroline Miles; (3) The Memory-span and Attention, by Dr. Arthur H. Daniels; (4) On the Least Observable Interval between Stimuli addressed to Disparate Senses and to Different Organs of the same Sense, by Miss Alice J. Hamlin; (5) Notes on the Binocular Stroboscope, a Model of the Hemispherical Field of Regard, and Diagrams for an Optical Illusion, by E. C. Sanford. [All of these papers are published in full in the *American Journal of Psychology*, Vol. VI., No. 4, Jan., 1895.]

(3.) *The Psychic Development of Young Animals and its Physical Correlation.* By T. WESLEY MILLS, Professor of Physiology in McGill University, Montreal.

As the comparative method, embryology and the doctrine of organic evolution have revolutionized biology, it must be expected that they or their analogies will at least greatly modify modern psychology. To learn how and when psychic processes originate is a long step towards understanding them; and as these processes in animals lower in the scale than man are presumably simple, it is desirable that they be studied both in the mature animal and in the young developing one. Accordingly the writer has for some years been engaged in this task, and has now made fairly complete researches on the psychic development of the dog, cat, rabbit, guinea-pig, etc.

An attempt has been made to keep a record in the form of a diary, not only of psychic, but of contemporaneous physical changes. A special series of experiments has been made on the brains of young animals, with a view of determining when cortical localization is established, in what order, etc. This work is not yet complete. Incidentally,

the subject of localization in the mature animal has been investigated, and some generally accepted conclusions found unreliable, as well as others confirmed.

(4.) *On the Distribution of Exceptional Ability.* By Professor J. McKeen Cattell, Columbia College.

A study of the mental traits and of the works of great men forms an interesting chapter in psychology; and while we are undertaking to make psychology an exact science, it is an advantage to secure quantitative results. When anecdotes are published telling us that certain great men have inherited or bequeathed their talents, were insane, immoral, precocious, versatile or the like, it is of interest; but we sometimes imagine that other examples might be quoted with opposite results, or similar traits found in ordinary people.

We need to be able to affirm that a man, who has accomplished work making him eminent, is more likely to be insane (according to a proper definition of insanity) than the average man, in a given ratio; and that this ratio varies in such and such a way for men whose work or character was of a given definable sort. And so in all cases quantitative results should be secured. We should be able to say that a man who is a great painter is just so much more likely to be a great poet as well, than is a great soldier, or than is the average man.

The first requirement for such a study is a list of great men secured by an objective method. The 1000 most eminent men have been selected by collating the space given to them in different biographical dictionaries and encyclopædias. The method secures impartiality and an assignable degree of accuracy, it being possible to give a probable error to each man. The list, of course, only gives a man's place in contemporary interest, but this would agree closely with the average verdict of the best judges as to his importance in history. The exact composition of the list is not indeed a matter of much importance for the end in view, an objectively selected list of great men being what is wanted.

The list was shown at the meeting, curves were exhibited demonstrating the distribution in time and race of the 1000 men, and attention was called to some facts brought out by the curves.

(5.) *Sensibility to Pain by Pressure in the Hands of Individuals of Different Classes, Sexes and Nationalities.* By Dr. ARTHUR MACDONALD, Bureau of Education, Washington.

Tabular Statement of Results.

No.		Total No.	RIGHT HAND.			LEFT HAND.		
			No. requiring more pressure in r. h.	Totals in kilos.	Averages, kilos.	No. requiring more pressure in l. h.	Totals, kilos.	Averages, kilos.
1	2	3	4	5	6	7	8	9
1	American professional men, . .	20	14	74.50	3.72	5	65.25	3.26
2	American business men, ...	14	6	85.25	6.08	6	87.75	6.05
3	American women, non-labor'g class	27	13	93.25	3.45	6	91.83	3.38
4	English professional men, ...	17	9	88.50	5.20	6	87.25	5.13
5	English women, non-labor'g class	7	4	43.00	6.14	2	44.25	6.32
6	German professional men, ...	6	5	31.25	5.20	1	29.00	4.83
7	Salvation Army members, London, ...	8	6	73.25	9.15	2	51.00	7.62
8	Slum men in Chapel Rouge, Paris.	9	3	122.50	13.61	2	119.50	13.27
9	Boston Army of the unemployed, . .	34	16	332.50	9.77	14	333.75	9.81
10	Women in "Maisons de Tolance," Paris, . .	9	3	82.00	9.00	5	84.25	9.36
11	Epileptic patients, laboring people.	3	1	28.00	9.33	1	27.00	9.00
12	Odd ones, men, in Paris, ...	7	4	28.25	4.03	3	26.25	3.75
13	Odd ones, men, in different countries, ...	18	10	96.25	5.34	5	89.50	4.97
14	Men in general, .	142	76	1012.75	7.13	49	979.50	6.89
15	Women in general	46	21	230.50	5.01	15	233.08	5.06

The experiments reported were made incidentally upon different classes of people. Quite a number of university specialists interested in the subject were experimented upon.

The middle of the palmar fossa was chosen, and Professor Cattell's Algometer was employed.

Should these results prove to be generally true by experiments on larger numbers of people, the following statements would be probable: The majority of people are more sensitive to pain in their left hand (only exception is No. 10, cols. 4 and 7).

Women are more sensitive to pain than men (Nos. 14 and 15, cols. 6 and 9). Exceptions are: comp. Nos. 4 and 5, cols. 6 and 9. It does not necessarily follow that women cannot endure more pain than men.

American professional men are more sensitive to pain than American business men (comp. Nos. 1 and 2, cols. 6 and 9); and also than English or German professional men (comp. Nos. 1, 4 and 6, cols. 6 and 9).

The laboring classes are much less sensitive to pain than the non-laboring classes (comp. Nos. 1, 2 and 9, cols. 6 and 9).

The women of the lower classes are much less sensitive to pain than those of the better classes (comp. Nos. 3, 5 and 10, cols. 6 and 9). In general, the more developed the nervous system, the more sensitive it is to pain.

Remark.—While the thickness of tissue on the hand has some influence, it has by no means so much as one might suppose, *a priori;* for many with thin hands require much pressure (Nos. 5 and 10, cols. 6 and 9).

(6). *The Freedom of the Will.* By BROTHER CHRYSOSTOM, Manhattan College, New York.

The positive results of the latest studies of the will, through introspection and experiment, are in striking accord with the teachings of the Schoolmen. The appetencies of Aristotle have been replaced by conation, which, if considered in the form of attention, is either *unequivocally conditioned*, and then corresponds to the sensitive appetition of scholastic philosophy, or is *equivocally conditioned*, and then does not essentially differ from the volition of earlier philosophers. But since equivocally conditioned attention may include among the objects attended to even the attending subject, it must be a spiritual action, for matter is incapable of such

reflexive process. In other words, the attending mind is a rational soul. In this light *apperception* may be characterized as the distinctive quality of conation. But apperception supposes at least such intellective action as is contained in conception, and this in turn supposes sensation; and thus a point of contact is made with Münsterberg's theory.

Neither a purely autogenetic nor a purely heterogenetic theory of will accounts for all the facts. For conation is not a mere combination of sensations, nor a resultant of affection and sensation, nor does it consist in affection alone. Again peripheral excitation fails to account for the active element of conation, while exclusively central excitation overlooks external influence. We must then adopt a theory midway between the two extremes. Wundt, therefore, must be held to state rather the physiological correlate than the psychical fact.

The chief difficulty as to the freedom of the will is found in its connection with the law of causality, which law, however, belongs to the domain of metaphysics, only indeterminism coming within the limits of psychology. *Cause* essentially connotes the inflowing of the agent upon some subject. But *free* and *uncaused* are not synonyms. All action of the will is voluntary, yet not all its action is free. For although the presentation of pleasurable or painful objects to the will, *i. e.*, the motives, together with the agent's temperament and general subjective condition determine the spontaneous impulse of his will, yet it is a fact of conscious experience that he often can and does put forth at the same time an *anti-impulsive* effort. Only actions made under these conditions are rightly called free, and they imply essentially the power to will or not to will.

Yet the law of causality, even in that narrower meaning which obtains in the physical sciences, also applies to free actions in the mass, for we can determine with more or less probability what men taken generally will do under given circumstances. In conclusion, Wundt's assertion that a free act is necessarily an uncaused one, is virtually an admission that the will is superior to material force, and is therefore spiritual.

(7). *The Consciousness of Identity and So-called Double Consciousness.* By Professor GEORGE T. LADD, Yale University.

The questions in debate concerning the consciousness of identity and so-called double consciousness cannot be intelligently discussed without a critical examination of the conceptions involved. What then do we mean when we speak of a thing, or a mind, as remaining 'identical' or self-same, through various changes of state? To uncritical thought it doubtless seems as though some unchanging 'core' of reality belonged to every being of which we feel ourselves entitled to speak in this way. But philosophical criticism seems rather to assure us only of the proposition: *The real identity of anything consists in this, that its self-activity manifests itself, in all its different relations to other things as conforming to law, or to some immanent idea.*

From this it follows that change, in itself, is not inconsistent with identity being maintained. On the contrary, it is the very character of the actual changes observed or inferred which leads either to the affirmation or to the denial of identity. This principle may be applied to whatever is popularly called a thing, and also to those hypothetical elements of all material things, the so-called atoms.

When we turn to consider the peculiar identity of mind, we find that the affirmation of such identity can never be taken as a denial of change. Indeed, the very real being of mind seems dependent upon change,—in the form, namely, of successive states of consciousness. So that the variety and greatness of the changes experienced *may* heighten rather than diminish the reality and validity of the consciousness of identity, properly described and understood.

Now if we inquire in what consists this conscious identity, we see that it is, and can be, nothing but that which is given to consciousness, in all states of self-consciousness, of recognitive memory, and of reflective thinking about the Self. To have these states of consciousness *is* to be conscious of being identical and self-same. And degrees of the consciousness of identity, as it were, are connected necessarily with all real mental development.

In accordance with this metaphysical analysis we may hopefully, and even confidently, venture upon the attempt to account for the phenomena of so-called double consciousness, in accordance with certain well-known psychological principles. Of these one may be spoken of as the principle of 'psychic automatism.' Under this principle we note in many of our most familiar experiences such a diremption of successive states, or of very complex present states into two-fold combinations of elements, as makes the full impression of *two* interacting personalities, rather than of one person. Yet very subtle and unrecognized or dimly recognized influences of one upon the other, of the Self-conscious Ego upon the automaton, or the reverse, may be distinguished by psychology. All this is popularly expressed either by saying, 'I have the automaton,' or 'the automaton has me;' 'I am the automaton,' or 'the automaton is not me.' Illustrations of all this may be derived from the simpler or more complex bodily operations as under the influence of semi-conscious states, and in turn influencing them; from many deeds of skill and valor, and even of a seemingly high order of intelligence; from the phenomena of artistic and religious inspiration, etc.

Closely akin to this is the most effective working of another principle, which we will call that of a 'dramatic sundering of the Ego.' We can more or less consciously and intentionally, or as forced by circumstances, so 'put ourselves into' another character as virtually to divide the Self into two or more selves, whose appropriate states of consciousness either follow in rapid succession or seem to occur almost simultaneously. The phenomena of dreams, the plays of children, the experience of many actors, the phenomena of certain states of inspiration, the imaginative genius of certain writers, like Balzac notably, are instances in point here. Indeed, the very nature of ethical consciousness, in its highest form of manifestation, necessarily seems to involve such a dramatic sundering of the Ego. In not very infrequent cases, three interacting personalities become manifest in consciousness. These may be described as the

tempter, or bad angel, the good angel, and the Self as the 'torn one,' between the two.

In fine, it seems fair to expect that by a further understanding and more extended application of these, and perhaps other cognate psychological principles, even the most extreme hypnotic cases of so-called double-consciousness may finally be explained.

(8.) *A Preliminary Report on a Research into the Psychology of Imitation.* By Prof. JOSIAH ROYCE, Harvard University.

This report first briefly described a collection of experiments now under way at the Harvard Psychological Laboratory, and then passed to some reflections, suggested by these experiments, relating to the definition of the functions to be grouped together under the name of Imitation. As the text of the report is to appear in THE PSYCHOLOGICAL REVIEW, the present summary need not be extended. The experiments, which at present are only in their first beginning, have thus far been confined to the imitation of somewhat complex series of taps, given by an electric hammer, and arranged in rhythms. The subjects of the experiments imitate the taps, after hearing each rhythm, through repeating the hammer-strokes by means of an electric key. The rhythms, as given and as imitated, are recorded on the kymograph. The effects of habit, in successive imitations of the same rhythm, the influence of speed, and of other factors upon success in imitation, are under study. The complexity of the rhythms studied in these experiments forms one special difference of this enterprise when compared with other experimental studies of rhythm. For the purpose is to study, not the rhythmic consciousness as such, but the imitative functions.

Notes of subjective experiences, taken down during or immediately after each experiment by the subjects concerned, have already given the suggestion for those considerations concerning the definition of imitation with which the major part of the report was taken up.

(9.) *The Classification of Pain.* By Prof. CHARLES A. STRONG, University of Chicago.

This paper was a discussion of the current theory that pleasure and pain are always given as aspects of a content distinct from themselves—the feeling-tone, 'quale,' or aspect theory. It sought to test this theory by considering its application to the case of cutaneous pain.

(1) Neurologically, we know no facts in regard to cutaneous pain which decisively contradict the theory. For special pain-nerves are more than doubtful; and there is a symptom of locomotor ataxia, consisting in hyperalgesia to heat or cold without hyperalgesia to pressure and even with analgesia to pricking and pinching, which seems to prove that some pains are distinctively pains of temperature. The condition of analgesia, moreover, while it implies distinct paths for pain in the spinal cord, may be reconciled with the aspect theory by holding that the sensations called forth through these paths is a tactile or temperature sensation in painful phase.

(2) But, introspectively, it is impossible in certain cases to carry out the analysis for which the aspect theory calls. Extreme pressure, heat and cold produce the same sensation—a sensation not of heat or cold or pressure, but simply of pain. This sensation (*Schmerz*) does not admit of analysis; it is impossible to separate it into a content and an accompanying feeling-tone. But it may call forth an emotional reaction in the shape of a feeling of the disagreeable or intolerable (*Unlust*).

In conclusion, the inference was drawn that pain, being a sensation, may be localized and may leave behind images.

[The paper will be printed in the PSYCHOLOGICAL REVIEW for May, 1895.]

(10). *A Theory of Emotions from the Physiological Standpoint.* By Prof. G. H. MEAD, University of Chicago.

Prof. Dewey having shown that it is possible to make a complete teleological statement of the emotions along the line of the discharge theory, it is interesting to see how far such a statement may be paralleled by a physiological theory.

This would involve, also, a physiological theory of pleasure and pain. As pain can be differentiated from the sensations in connection with which it generally appears in consciousness, as it shows itself under circumstances in which the tissue of the end organs or the nerves themselves are affected, and as in the diseases in which we find pain as a constant concomitant, those parts are affected, which are richly supplied with blood vessels by means of supporting and nourishing tissues (*Rindfleisch's intermediärer Ernährungsapparat*), and as in those diseases which pass usually without pain (as in the catarrhs of the various mucous membranes) the tissues affected are poorly supplied with such blood vessels, and enter into relation with the capillaries generally through the lymph, for the purposes of secretion, it becomes at least probable that, physiologically, pain may be considered as the interference through poisons or violence or otherwise with the process of nutrition as carried out in the finer arteries and blood vessels. Pleasure must from this standpoint be considered as physiologically the normal or rather hightened process of nutrition in the organs, and the nerve paths which connect these with the central nervous system would be probably the sympathetic.

In the simple instinctive act that lies behind every emotion, the vaso-motor system is called into action by the enlargement of the small blood vessels in the muscles and sweat glands. To maintain the blood pressure the finer blood vessels in the abdominal tracts are closed by the constrictors of that region, and the action of the heart may also be increased by the accelerators. The vaso-motor system thus is, in these simpler instinctive acts, in automatic connection with the senso-motor. The act must commence before the flow of blood can take place. It is in connection with this increased flow of blood that we have to assume the emotional tones of consciousness arise according to the discharge theory. Within the act it would answer only to interest. It is in the preparation for action that we find the qualitatively different emotional tones, and here we find increased flow of blood before the act. We find also what we may term symbolic stimuli, which tend to arouse

the vaso-motor processes that are originally called out only by the instinctive acts. These stimuli in the form in which we can study them, seem to be more or less rhythmical repetitions of those moments in the act itself which call forth especially the vaso-motor response. In this form they are recognized as æsthetic stimuli, and may be best studied in the war and love dances. It is under the influence of stimuli of this general character that the emotional states and their physiological parallels arise. The teleology of these states is that of giving the organism an evaluation of the act before the coördination that leads to the particular reaction has been completed.

(11). *Desire as the Essence of Pleasure and Pain.* By Dr. D. S. MILLER, Bryn Mawr College.

Pleasure and pain, in the discussion now going forward as to their classification and physical basis, are commonly treated as among our passive sensory experiences; at all events, it would seem to most psychologists a somewhat stupid paradox to assert that they were in any sense motor phenomena. Yet there is solid ground for holding this paradox; for maintaining, at least, that pleasantness (the quality which, along with their specific differences of character, marks all so-called pleasures) and painfulness (the quality which, along with their specific differences of character, marks all so-called pains) are essentially motor facts. A pain is an intolerable feeling; different as they are among themselves, all pains have this, at least, in common, that they are intolerable. No other feeling is intolerable; if it were we should call it a pain. It would, then, not be easy to refute the proposition that painfulness *is* intolerableness; that so-called pains have no other common class-attribute. Now intolerableness is the quality of uniformly provoking a certain bodily disquietude or rebellion, issuing, where the nature of the case permits, in an attempt to escape from the offending irritant. And this is a motor phenomenon. The various *disagreeables* (a term with which 'pains' in my meaning is convertible) a needle-prick, a headache, a burn, the numb internal ache of cold hands, the taste of quinine, the

smell of assafœtida, the scratching of a slate-pencil, 'gnawing pains,' 'shooting pains,' muscular fatigue, disappointment, humiliation—these have no such intrinsic resemblance in sensational complexion as we find among different sights or sounds—between the members of the class of visual, or of the class of auditory sensations; they are similar only in the extrinsic fact that they all alike are accompanied by a bodily reaction—some flinching or shuddering or convulsion, some restiveness or inner tension—which tends then and afterwards to pass into movements of avoidance, escape or repulse. Now these movements and the tendencies to them are what we know as *aversion* in its various forms and degrees.

If painfulness is intolerableness, pleasantness, on similar grounds, is the quality of being *welcome*. The bodily reaction of *gusto* is as characteristic, though not so obtrusive as that of intolerance; and it tends to pass into movements of retention or procurement. These movements and the tendencies to them are what we know as *desire* in its various forms and degrees.

(12.) *Pleasure and Pain Defined.* By Prof. SIDNEY E. MEZES, University of Texas.

It is necessary to find some fact or group of facts that is present whenever we experience pleasure and absent whenever we do not, and another fact or group of facts present and absent with pain. The frequent confusion of unpleasants with pains is very misleading. Unpleasants are of three kinds: memories and expectations, sensational unpleasants that are not pains—bitter tastes, *e. g.*—and sensational unpleasants that are pains—a toothache, *e. g.* We have here to define pleasure and the unpleasant. Attempts have been made to define pleasure-pains as sensations, as emotions, and as making up the genus of which sensations and emotions are two species. The fact that there is evidence for each of the first two theories shows that neither is exhaustive and competent. Besides the existence of pleasant and unpleasant memories, expectations and fancies invalidates all three. Many hold that pleasure-pains are ultimate

ideas, simple and undefinable, like colors. There are strong positive objections to this theory, but negatively, and for our purposes, it suffices that this theory is a last resort, and that its supporters must overthrow all other theories before legitimately claiming it as established. This theory is valuable and true in so far as it points out that neither pleasures as a whole nor unpleasants as a whole have any properties in common. It overlooks the possibility that there may be something invariably co-present with pleasures and some other invariably co-present with pains; and that these two may be the signs to us of the presence of pleasures and pains,—what induces us to call a state pleasant or unpleasant. Now Plato, Aristotle, Hobbes, Kant, and Schopenhauer agree that harmony or good adjustment is the mark of pleasure, ill-adjustment that of pain. Not all these writers point out the terms between which the adjustment is to obtain, but recently Wundt and Ward have held that the adjustment is of attention to its object. This immediately plausible suggestion of attention and adjustment must be examined. Clearly what is not attended to is indifferent since uninteresting. Further immediate attention to pleasures is not the same as that to pains: the former is easy and natural, the latter enforced and obstructed. Again derived attention, always to unpleasants, is invariably obstructed by the more pleasant rivals to attention also present. May it not be that attention without obstruction is the mark of pleasure, attention with obstruction that of pain? The evidence for this view may be thus suggested: All states of intensely concentrated attention are pleasant, hard thinking, hard play, strenuous work; all states of *internal* conflict—hesitation, practical puzzle, co-present irreconcilable impulses, morbidly insistent ideas, etc.—are unpleasant; and further physical pains, owing to their great intensity, reverberate widely and naturally set up mutually obstructive reflexes. The paper appears in the *Philos. Rev.*, Jan., 1895.

(13.) *Emotions versus Pleasure-Pain.* By Mr. HENRY RUTGERS MARSHALL, New York.

Mr. Marshall reviewed his 'genetic' argument in relation to the Emotions, emphasizing the contention that the

typical Emotions are named because (1°) they correspond to relatively fixed relations between the physical elements reacting, and because (2°) these reactions are immediate. Failure of these two conditions can be traced where 'instinct feelings' have no emotional names. Emotions are in their nature irregular in recurrence, and to be of value must be forceful in reaction; hence Emotions are not usually lost to consciousness as many 'instinct feelings' are, although, if these Emotions become rhythmical and weak, they act as other states do in relation to fixity of habit. Pleasure and pain relate to *organic*, while Emotions relate to *individual or racial*, effectiveness or ineffectiveness; therefore their genesis cannot be considered to have been coincident in time, nor to be of the same type.

The identification of Emotion and Pleasure-Pain in 'Feeling' is dependent upon the validity of the tripartite division of mind; which is upheld by metaphysical postulation but not by psychological evidence. Prof. Croom Robertson argued that the exhaustive categories, The True, The Good, The Beautiful, themselves proved the validity of the division. But the existence of the division is explicable in quite another way, as due to the search for Reality. In relation to mental experience in general, this search gives us the True; in relation to Impression, it gives us the Beautiful; and in relation to Expression, it gives us the Good. If we are to discard this classical tripartite division, we should be able to account for its persistence. It results from an attempt to unify two diverse classifications, both bipartite; viz., 1°, the receptive-reactive classification, and, 2°, the subjective-objective classification: Sensation and Intellect (knowing) being bound together on both the receptive-reactive and on the subjective-objective schemes; Pleasure-Pain and Emotion (feeling) being bound together on the subjective-objective scheme, the receptive-reactive quality being unmarked; Will being marked by a common and coördinate emphasis of the reactive and also of the objective qualities. The existence of this tripartite division, thus explained, can therefore no longer be used as an argument for the bond between Emotion and Pleasure-Pain, which states are dis-

tinctly separable, the relation between them being this: The Emotions are complex psychoses which almost invariably involve repressions or hypernormal activities, either of which are determinants either of pleasure or of pain.

(14.) *Notes on the Experimental Production of Hallucinations and Illusions.* By Prof. W. ROMAINE NEWBOLD, University of Pennsylvania.

Dr. Newbold reported that in 22 out of 86 cases tried he had succeeded in producing illusions by causing the patients to gaze into a transparent or reflecting medium, such as water, glass, and mirrors. His most successful cases were found among young women under twenty years of age who were good visualisers, but as a majority of his subjects were young women, and as the experiments were by preference made upon good visualisers, he was not inclined to lay much stress upon these conditions. The phantasm was usually preceded by cloudiness, flushes of color or of light in the medium, and varied from a dim, colorless outline to a fully developed and brilliantly colored picture. The images were frequently drawn from the patient's recent visual experience, were sometimes fantastic and frequently unrecognised. The successive images were usually associated, if at all, by similarity, but frequently no relation could be discovered between them. Association by contiguity was excessively rare. The phantasm was frequently, but not always, destroyed by movements of the medium and by distracting sensory impressions and motor effort. Occasionally the phantasm was to a considerable degree independent of the medium, persisted for some time after the removal of the medium, and in one such case appeared to obey the laws of the after-image. The importance of such phenomena upon the question as to the value of the central component in the after-image is obvious.

No trace was observed of telepathic or other supposed supernormal agency. There seemed to be no reason for regarding the phantasms of the glass as anything other than illusions of the ordinary types depending upon the glass as a *point de répère*. Their chief speculative importance, apart from the light which they may throw upon the after-image,

lies in the fact that they present to us processes of association by similarity in concrete, sensible form, and in their possible relation to subconscious 'automatic' processes. While the phantasms as such cannot be regarded as demonstrating the existence of such processes, it is probable that, if subconscious automatism exists, its products may be traceable in the phantasms of the glass. It is possible also that some specific relation exists between the hypnotic consciousness and the phantasm of the glass. Dr. Newbold found that images unrecognised by the waking consciousness were sometimes recollected by the patient when hypnotised, and, *vice versa*, experiments by Mr. F. W. H. Myers have shown that a tale related in hypnosis is sometimes presented in the glass externalised in dramatic form.

[This paper is to be printed in full in an early number of the PSYCHOLOGICAL REVIEW.]

(15.) *Experiments on Dermal Pain.* By HAROLD GRIFFING, Ph.D., Columbia College.

By means of an algometer transmitting pressure up to 15 kilog. the average pain threshold was found to be for 40 college students, 5.5; for 38 law students, 7.8; for 98 women, 3.6; for 50 boys, 12–15 years of age, 4.8. The palm of the hand was the place of stimulation. The most sensitive parts of the body are those where the skin is not separated from the bone by muscular and other tissues.

In 80 experiments on two observers the area was variable, areas of 10 mm., 30 mm., 90 mm. and 270 mm. being given. The corresponding average values of the pain threshold were 1.4 kilog., 2.8 kilog., 4.4 kilog. and 6.6 kilog. Thus the pain threshold increases with the area of stimulation, but much more slowly than in direct proportion.

The time in which dermal stimuli of different intensities cause pain was found by noting the time that elapsed before the appearance of pain after weights had been placed in a balance pan in such a way as to press upon the hand. The averages in seconds, based upon 80 experiments on two observers, are as follows: For 100 g., 230 sec.; for 200 g., 35 sec.; for 300 g., 10 sec.; for 500 g., 4.5 sec. Thus the time,

as well as the area and intensity of stimulation, are factors in dermal pain. There is, moreover, an intensive limit below which pressure stimuli never cause pain. Above this limit the sensory effect of the time seems to be in direct proportion to that of intensity.

The pain threshold for falling weights was found to depend as much upon the height as the mass. As both the height and mass are proportional to the kinetic energy of the moving mass, the stimulus for dermal pain in impact must be considered the energy of the striking object.

(16.) *The Normal Night-Blindness of the Fovea.* By CHRISTINE LADD FRANKLIN, Baltimore.

König's announcement in May, 1894, of the very close coincidence of the curve showing the distribution of brightness along the spectrum for (1) the totally color-blind and (2) the normal eye in a faint light, with the curve of relative absorption of different portions of the spectrum by the visual purple (and the obvious inference therefrom that the vision of the totally color-blind and that of the normal eye in a faint light are conditioned by the presence of the visual purple in the retina) made necessary some assumption to take account of the fact that no visual purple has hitherto been found in the fovea. Two assumptions were possible,—either that the cones (and hence the fovea) do contain visual purple, but of such an extremely decomposable character that it can never be detected objectively; or, that the eye of the totally color-blind person, and the normal eye in a faint light, are actually blind in the fovea. As I had already made the prediction that total color-blindness consists in a defective development of the cones of the retina (*Ztsch. f. Psych. u. Phys. der Sinnesorgane, Bd. IV.*, 1892) and also that the adaptation which renders vision possible after twenty minutes in a faint light is conditioned by the growth of the visual purple (*Mind*, N. S. III., p. 103)—both predictions being naturally suggested by my theory of light-sensation—I was most anxious to put the latter assumption to the test. I therefore undertook to determine, in the dark rooms of Prof. König's laboratory, the threshold for light-sensation for dif-

ferent parts of the retina and for different kinds of monochromatic light (the full results of this investigation will appear later). The blindness of the fovea for faint light did not at once reveal itself; the act of fixation means holding the eye so that an image falls on the part of the retina best adapted for seeing it, and hence it would involve keeping the image *out of* the fovea in a faint light, if the fovea were really blind in a faint light. But after the total disappearance of the small bright object looked at had several times occurred by accident, it became possible to execute the motion of the eye necessary to secure it at pleasure. It was then found that the simple device of presenting a *group* of small bright objects to the eye of the observer was sufficient to demonstrate the 'normal night-blindness of the fovea' (as it may best be called) without any difficulty,—one or the other of them is sure to fall into the dark hole of the fovea by accident. It was only by means of this arragement of a number of small bright spots that the total blindness in the fovea of the totally color-blind boy could be detected,—he had, of course, learned *not* to use his fovea in fixation. Prof. König then proceeded to demonstrate the total blindness in the fovea of the normal eye to blue light of wave-length about $\lambda 470$.[1] [These experiments upon the normal eye were exhibited at Princeton.] It was shown that König's proof that the pigment-epithelium is the only layer of the retina which is affected by red, yellow and green light is not wholly conclusive. The interpretation of the new facts, and their bearing upon the several theories of light-sensation, were discussed. [This paper appears in full in the PSYCHOLOGICAL REVIEW for March, 1895.]

(18.) *The Muscular Sense und its Location in the Brain Cortex.* By Prof. M. ALLEN STARR, New York.

[This paper was presented in the absence of Prof. Starr. It may be found in full in the number of the PSYCHOLOGICAL REVIEW for January, 1895.]

[1] Prof. v. Kries is said to have shown that the experiments in question do not establish the blue-blindness of the fovea (*Berichte der naturforschenden Gesellschaft zu Freiburg*, IX., 2, S. 61). I have not yet had access to this criticism.

(19.) *Psychology in the University of Toronto.* Prof. J. G. HUME, University of Toronto.

In the University of Toronto we begin the work in Psychology, etc., in the Sophomore year. Up to that time the students are engaged in language studies, mathematics, English history, chemistry, biology, etc. After the Sophomore year they still continue some of this language study as supplemental to the philosophical course. The latter, beginning with psychology, logic and theory of knowledge in the second year, psychology, logic, theory of ethics, history of ethics and history of philosophy in the third year, keep extending until, in the fourth year, those who have selected this course give *all* their time to the subjects of the course without any supplemental work, taking, in the fourth year, psychology, ethics, history of philosophy, special reading in the original of various selections from the whole period of modern philosophy, giving special attention to Kant and Lotze.

In experimental psychology: Second year, 2d part of the year: Demonstrations from the Director, explanation of methods and practice. In the third year, during the whole year, the class, divided into groups, is under the charge of the Director of the Laboratory. In the fourth year they are supposed to be able to undertake experiments of an independent character. Some of the inquiries started in the fourth year are continued in post-graduate work.

In the present fourth year there are sixteen honor students conducting four sets of experiments, that is, in four groups, with four in each group: I. On Time reactions (Mechanical registration instead of the Chronoscope); II. Discrimination of Geometrical Figures and Letters in the Field of Indirect Vision; III. Discrimination of Color-saturation; IV. Discrimination and Reproduction of Rhythmic Intervals. In post-graduate study there are two enquiries being continued from last year: I. Estimation of Surface-magnitude; II. On Certain Optical Illusions. The Director of the Laboratory, Dr. August Kirschmann, has in the press a recently finished investigation upon the nature of the perception of *metallic lustre.*

[This paper was presented in the absence of Prof. Hume.]

DISCUSSION.

THE SENSATIONS ARE NOT THE EMOTION.

The tendency to assume that the peculiar sensations involved in any psychological fact are the fact itself, comes out strongly in the present discussion over emotion.

When it is shown, for instance, that apart from certain visceral and vaso-motor sensations there is no emotion worth speaking of, we are asked to view emotion and these sensations as identical. Why should we not, quite as well, take emotion to be merely a flutter of thought or a special aspect of attention? Apart from these, there is likewise no 'coarse' emotion.

In fact emotion requires the bodily sensations, but it requires them to be under definite mental conditions which are as indispensable as the sensations themselves. In the first place, some interest which will divert the attention so that these sensations may play the part of mere 'fringe,' is doubtless an important condition for the life of the sensations, but it is also more. Such an interest keeps the sensations in a peculiar relation to the whole mental field. So that the sudden loss of emotion when attention is turned to the body, is probably due less to the fading of the essential sensations than to their reversed relation in the general mental state. Momentarily even during strong excitement, so my observation goes, we can glance at the bodily commotion while many of its most striking elements continue vigorous. We may even cut down between them and us, viewing them as outsiders, as confusion of the body and not of the thinking itself. Instead of strong emotion the state instantly changes to one, say, of psychological query not markedly emotional. The next instant the attention is away, the sensations surge back over the thought, the point of interest is seen *through* the confusion, and the state is unmistakably emotional. As far as I could make out, the sensation-substrate of the two states is about the same, and yet the states themselves are decidedly different.

In emotion we feel that there is confusion in *us*,—in *this* end of the relation. But when we turn upon the bodily sensations themselves, the confusion seems to go over to the other end of the

relation. The object of attention now is in turmoil, but the thought-process itself may for a moment be comparatively calm. The state then need not be emotional, though the object watched is disturbed enough.

It is difficult to give the facts in less figurative language. But substituting the classic figures, we may say that for a state to be emotional it requires a special character of 'form' as well as a special character of 'matter,' whether this matter be taken as sensation or 'tone' or both. In the general upheaval, the operations which relate the sensations are usually more or less disordered. The central nervous processes act spasmodically. Thought is wavering, and the confused bodily sensations seem part and parcel of the confused thinking. But these sensations are by no means equivalent to the emotion. They are merely one abstract aspect of the emotion, of which other important (though likewise abstract) aspects are the rush and whirl of thought, and the special relation of the sensations to the mental field.

LEIPZIG. GEORGE M. STRATTON.

A CORRECTION.

On p. 72 of the last number of the PSYCHOLOGICAL REVIEW an omission was made in my abstract of the Sidgwick report on hallucinations which makes the calculated figure of 1300 on line 9 from the top of the page unintelligible. The figure calculated from the premises which I quote is 1400, for which my text substitutes 1300 with no motive assigned. The motive obeyed by the authors of the report is the probable untrustworthiness of accounts of apparitions falling within the first ten years of the informant's life. Such visions are subtracted by the committee both from the total number of recognized apparitions and from the number of coincidental apparitions [See Proceedings of S. P. R., pp. 65, 247]. They form 8 per cent. of the former, so that my abstract of the calculation should have dealt with $\frac{92}{100}$ of 350 instead of 350. This makes 322, a figure which multiplied by 4 gives 1288. For this the committee substitute 1300, as a 'round number,' slightly more favorable to the adversary. W. J.

PSYCHOLOGICAL LITERATURE.

Philosophical Remains of George Croom Robertson. With a Memoir. Edited by A. Bain and T. Whittaker. London, Williams & Norgate, 1894. Pp. XXIV + 481.

In this volume we have, with the exception of the little book on Hobbes and one or two historical articles, all Robertson's philosophical writings. This goodly volume, however, is more than half composed by the republication of shorter articles, critical notices, and notes from *Mind*. An outsider may well wonder why a good deal of this should have been reprinted. The explanation is simple. Robertson held a high reputation in England as a teacher of philosophy. This reputation owed something to the startling and sensational manner in which he appeared on the philosophic scene, when, as a youth of 25, and quite unknown beyond his own Scotch University, he was elected Professor of Philosophy at University College, London, over the head of Dr. James Martineau. This election, due to the strong backing of his teacher Dr. Bain, and Bain's friend George Grote, showed at least that there were some who expected high things of the Aberdeen youth. And their expectations were not disappointed. Robertson proved to be an excellent teacher, endowed with the peculiar gift of guiding the young learner into the labyrinth of philosophic complexities by help of a few well defined clues. To some his lectures were too elementary, and moved too slowly, but to the average student they were exceptionally helpful. He soon began to be known in London society as an authority on philosophical questions. He was a member of the oddly-named Metaphysical Society, the *raison d'être* of which is said to have been the desire of Lord Tennyson, expressed to his faithful attendant, Mr. James Knowles, to ascertain whether he had a soul, though it soon became evident that the experts, viz., the theologians of all creeds and the scientists, who were called in to decide the great question, were much more concerned to attack one another's views. Robertson could hardly have felt quite comfortable here, yet he managed to get this 'metaphysical' *omnium gatherum*, or rather a portion of it, to listen to one or two papers of his own. Outside this society his influence steadily

grew. His appointment to the editorship of *Mind* in 1876, when that journal led the way among English and American philosophical serials, greatly widened the sphere of his influence. This brought him later on into touch with Prof. W. James and other Americans as well as with French and German thinkers. The present given to him by contributors on his retiring from *Mind* two or three years ago showed how warmly he had attached many by his excellent conduct of the journal. For some years his house was the rallying point of the small band of philosophic students of which London could then boast. Leslie Stephen, Shadworth Hodgson, F. Pollock (not then the baronet), F. Gurney, F. Galton, myself, and others, were often to be found there. W. James joined the circle the winter he remained in London. At this time Robertson's talk, which in spite of an occasional smack of the cathedral manner, was distinctly good, gave him prominence in such social gatherings. There was an energy, an alertness, tempered by an Aberdeen 'canniness,' which made him impressive, and he often had a happy way of cutting into a dialectic tangle and extricating the point of real importance. A painful illness was soon to compel him to retire from much of this old social life.

It was necessary to say so much about Robertson's personality as well as his teaching and editorial work in order to explain these *Philosophical Remains*. For a glance at them tells the reader that their collection is the outcome of a feeling of piety. But for this we certainly should not have had reprinted some of the critical notices which in these days of rapid psychological advance already look out of date. The truth is, as he more than once confessed to me, Robertson was not a ready writer. This indeed betrays itself in the literary manner, which, though it has a decided character and certain good qualities, is apt to become awkward even to the point of contortion. The very pains-taking to be clear, to limit a statement to the dimensions of strict accuracy, ended by destroying smoothness.

While there were these half mechanical difficulties in the way of literary production, there was I think another reason for its paucity. There is no evidence that Robertson was ever fully possessed by an impulse to write a considerable philosophical work. The work on which he was supposed, for many years after his appointment at University College, to be engaged, was a study of Hobbes. His little book, which appeared in the 'Philosophical Classics' series, into which the results of these years' study were compressed, shows no doubt careful scholarship, and close critical study of his subject and its historical relations. Yet it does not I think suggest any large

and important originality of thought. It strikes one in reading these *Remains* that Robertson had the freshness of view that goes to make a critical expositor and teacher rather than a true constructive originality. As a teacher he was never so happy as when reading and expounding some philosophic classic to one of his small class of advanced students. The very fact that his one book was mainly a historical exposition seems to say that his bent lay in the direction of philosophic exegesis and of historical criticism. The same impression is, I think, borne out by the *Remains*. The best critical notices seem to me to be those of works on the history of philosophy. Other articles, not dealing directly with the history of the subject, show the same tendencies. Thus the excellent article on 'Axiom,' and in a less degree also the other longer articles reprinted from the *Encyclopedia Britannica*, *viz.*, 'Analysis,' and 'Association of Ideas,' show how Robertson's strength lay in what one may call the expository clarification of ideas. The way in which the word 'Axiom' has come to mean the different things it does is admirably traced out by a happy combination of accurate historical learning and logical coordinative power. These papers are in their way models of Encyclopedia articles. They show the same qualities, accuracy, perspicacity, grasp, and, what is equally important, a clearly recognizable method, which helped to make him an eminent teacher.

It is time, however, in writing a notice of Robertson's work, for a psychological journal to say something about his work on psychology. As the most distinguished pupil of Alexander Bain, for many years the commanding influence in British psychology, as the hearer of Lotze and other distinguished Europeans, Robertson was always looked on, more than anything else, as a psychologist. And this way of regarding him was in the main justified. He put forward in an admirably clear and convincing manner the claims of psychology to be the propædeutik among philosophers' disciplines. The position is made clear in the introductory lecture which he gave on his appointment to the chair of University College, and is made still clearer in an article on 'Psychology and Philosophy,' published in *Mind*. The establishment of this journal which, as Bain generously allows in his far too short memoir prefixed to the *Remains*, was in considerable part Robertson's work, was intended, as its title and its editorial preface clearly showed, to give fundamental prominence to psychological work and thought, and this intention was never lost sight of. Although there was no experimental psychology in England, and the later experimental work in America had not begun, Robertson managed to get together a good deal of valuable

contribution ; so that *Mind* will long remain an important work of reference for psychologists. His own contributions to the journal show that his mind was fairly engaged with all the newer researches, psychological and physiological, which bear on the understanding of mental processes. Here, however, one recognizes rather the skill with which newer results are brought into relation to older ideas than original contribution, the setting forth of new and luminous psychological ideas. Now and again no doubt there is an attempt to elaborate a new conception, as where in the article on 'Axiom' he seeks to apply the muscular theory of space-consciousness to the problem of mathematical axioms and to show that (as Kant said in his way) by "acting constructively in our experience, both of number and of form, we, in a manner, *make* the ultimate relations of both to be what for us they must be in all circumstances" (p. 129). More ambitious is the attempt to get over the difficulty of the genesis of space-consciousness by saying that we know thing or object as resistant before we know extension or space, that the successions of muscular experience by which we come to know extension, somehow get transformed into the intuitive of space by being referred to the more fundamental object-intuition (p. 279 ff.). It is not quite easy to seize Robertson's exact drift here. Much of what he writes here looks as if he thought the psychologists' task was to explain the objective reference of a space-consciousness already existent, rather than to account for the form or structure of this space-consciousness itself. Yet while these contributions to psychological discussion are not as impressive as one might have expected, they are fresh and suggestive, and they make one regret that Robertson did not give a fuller statement of his views on other perplexing points.

Yet Robertson's friends, at heart, will value this volume as a reflection of the mind and, in some respects, of the character they knew and valued. If it gives us no striking contribution to the field of modern psychological research, it shows us the eager and patient spirit resolved to track ideas to their sources and their elements ; it shows us the born teacher to whom luminous apprehension of truth must express itself in no less luminous an exposition. Such men are as great benefactors as the writers of works. Robertson's devotion to philosophic work, which, as Leslie Stephens' enthusiastic letter tells us, became almost heroic, when for years it had to contend with most unstable health and bouts of prostrating physical suffering, deserves a permanent record in America hardly less than in England.

UNIVERSITY COLLEGE, LONDON. J. S.

Lectures on Human and Animal Psychology. W. WUNDT. Translated by J. E. Creighton and E. B. Titchener. London, Sonnenshein; New York, Macmillan. 1894. Pp. X+454.

A few years ago a new edition of Professor Wundt's celebrated book *Vorlesungen über Menschen-und Thierseele* appeared. The first edition was written thirty years ago at a time when the problems of empirical psychology had just begun to be realized in all their significance. Since that time psychology as an experimental science has greatly developed; it has adopted and devised exact methods of research; it has followed out carefully many investigations and proved by the results, that the same mathematical accuracy, with which natural sciences like physics and astronomy carry out their work, may be applied successfully to the natural science of mind. The necessity of a psychological way of viewing the facts besides the physical is in our days universally acknowledged. The new edition of the book mentioned is thoroughly revised by the addition of the results of recent investigations and by the omission of every thing which has not stood the test of greater light.[1] In size the book has been considerably reduced, by dropping those discussions which have now developed into a certain independence as special sciences, such as Social Psychology. The book is arranged in thirty lectures, the first thirteen treating Sensation and Presentation. In Lects. 14 to 20, the Feelings and their Relations, the Theories of Association and Apperception are treated. The last part of the book, Lects. 21 to 30, deals with the more complicated problems of animal and human psychical life: Mentality of Higher Animals, Development of Intellectual Functions, Instinctive and Voluntary Actions, Mental Disturbances, Dreams, the Hypnotic and Posthypnotic phenomena, etc., closing with a discussion of the ultimate questions of psychology and their philosophical bearing.

Messrs. Creighton and Titchener are fortunate in having furnished us with a carefully prepared and excellent translation of this book into English; and many who wish to become acquainted with the ideas of this German philosopher, whose efforts have brought about such wonderful advancement of psychological science, will entertain a sense of gratitude to the translators for saving them the trouble of seeking their way directly and in the original language through the more difficult books of the same author.

TORONTO UNIVERSITY. A. KIRSCHMANN.

[1] A great improvement is also seen in the addition of many good illustrations.

Primer of Psychology. By G. T. Ladd. New York, Charles Scribner's Sons, 1894. Pp. IX + 224.

Professor Ladd's Primer is like his other books in that it is carefully written, systematic, and embodies the latest results of psychological research. It wisely leaves out metaphysical discussion, confining itself to psychology proper, a feature of especial importance in view of the class of readers to which it is primarily addressed. Its style is somewhat unequal, being in parts quite simple and adapted to immature students, and in parts rather burdened with scholastic terms which might, I think, have been avoided. It is not, however, with the use of technical terms that I chiefly quarrel. One may use a rather large number of such terms and yet write in a style which is plain, easy, and entertaining. A book which bears the title of 'Primer' should be written in such a style. It should be fresh and unstilted, free from all flavor of scholasticism,—it should not smack of the professor's chair. Such books are not easy to write. They can not readily be thrown off as "a recreation between two much more bulky and serious pieces of work." They require great skill, not only in the selection of material, but also in the exposition of the material selected. They call for a rare insight into and a rare sympathy with the ways of thinking of young and immature minds, so easily repelled by what is 'dry,' and discouraged from further effort. Learning may be rather a hindrance than a help in the writing of such books. It may separate one too far from the class of readers one wishes to reach.

In the present instance I cannot but think that the above criticism may with justice be applied. I do not mean to make the criticism at all a severe one, for Professor Ladd's book is usually clear and is well arranged. I should not hesitate to use the 'Primer' with a class of young people. But I have felt in reading it that it is dry, and that the writer lacks that peculiar gift—a very rare gift it is—of writing successfully for the young. To do this one must above all be fresh and simple and natural. One must forget one's learning, and with it the turns of phrase which are out of place in the 'Elements of Physiological Psychology' and the 'Introduction to Philosophy.' When one spends one's life among such, it is, of course, not easy to forget them.

Over the contents of the book I need not linger, as Professor Ladd's opinions are well known. I wish that in the chapter on feeling he had indicated more clearly the ambiguity of the word. Certainly the impossibility of describing what is meant by feeling (p. 53) cannot refer to the complex experiences to which the word is

applied a little later (p. 56). We can at least point out, as Professor James has so well done, some of the elements which enter into these. Such a pointing out of the elements in a complex is what constitutes description of a thing seen, though, in this latter case, the analysis is one more readily made. Again, it does not seem to me likely to aid one in clearing up the psychology of memory to state that "every act of memory with recognition transcends the present, and connects the present into a known real unity with the past;" and, having thus stared the difficulty boldly in the face, to pass on with the remark that this is one of the profoundest of all mysteries. The mystery is, I think, not psychological, but, if it exist at all, epistemological. In psychology we are concerned only with the question, "What mental elements are actually present in a given mind when it recognizes something?" These elements we may not be able to enumerate, and in so far we may call them mysterious; but when the problem is stated psychologically it does not, I think, present so hopeless an aspect as it does when stated as it is by Professor Ladd.

In looking over the above I find I have made my criticism more negative than I had intended. I have not dwelt upon the merits of the 'Primer' as much as I have upon what appear to me its shortcomings. It was perhaps as well to do this, for it goes without saying that a new book by Professor Ladd should have the strong points which characterize his other books. The author has been too long in the field, and is too well known, to make it necessary to praise him. G. S. F.

The Elements of Metaphysics. PAUL DEUSSEN. Translated from the Second Edition by C. M. Duff. London and New York, Macmillan & Co. Pp. XXIV + 337.

Dr. Deussen, the author of the Elements, has been known for years as an enthusiastic student of Indian Philosophy and a representative of that school of Orientalists who reject the negative conceptions that have been historically associated with Buddhism and follow Cañkara as the true interpreter of Hindu Metaphysics. Dr. Deussen's own philosophical position is Kanto-Schopenhauerian. Kant he asserts was the first discoverer of the true principle of philosophy, while Schopenhauer alone has developed that principle truly and said the last word in metaphysics. This being the author's faith his work is on the whole a pretty faithful reproduction of the philosophy of these masters.

Dr. Deussen starts with a distinction between Science and Philosophy. The standpoint of science is empirical and materialistic. Materialism can be overcome only in the transcendental standpoint of philosophy which regards the world under the dual Kantian categories of phenomena and things in themselves. This Kantian distinction has been translated by Schopenhauer into corresponding subjective terms, intellect or reason, and will. The world is dual, it is a world of intellect and a world of will. Now, the intellect whose innate forms are space, time and causality is a purely phenomenal faculty through which the will projects a world in space and time and causal connection. But this world is appearance and not reality. The intellect is material in its objective constitution, being identical with the brain. But this whole world of the intellect is appearance and must be transcended in order that the world of reality may be reached.

The thing in itself, or real, is the will whose central motive is the striving for life or self-realization. This striving of will expresses itself as the Platonic ideas in the physical forces of the world, thus grounding the phenomenal world in its deeper dynamic aspect. Dr. Deussen makes a tripartite division of transcendental philosophy into the metaphysics of nature, of the beautiful, and of morality. The main ideas of the first division have been given above. In nature which also includes the ordinary phenomena of man, the will does not manifest its archetypes as they are in themselves, in their 'unspoiled form and beauty,' but only an adumbration of these. For the more adequate expression we must pass *first* to the Metaphysics of the Beautiful and *finally* to the Metaphysics of Morality. Nature is the expression of the affirmation of the will to life which is empirical, individual, egoistic. It can be transcended only by *denial* through which alone is a door opened into the heart of reality. Now, art in its feeling for the beautiful which Kant defines as a 'disinterested delight,' enters this door through a kind of self-forgetting of the will. The will is affirmative in its nature and does not care for things in themselves, but only as they affect it. But in the art-feeling this egoism drops temporarily out of sight and the will experiences a delight in that which has no reference to itself. This is a contradiction which art cannot explain and we are led on to morality for its solution. It is only in morality and religion that the phenomenal world and its contradictions are actually transcended.

Dr. Deussen's treatment of the metaphysics of morality embraces the following essential points. (1) The tripartite classification of the will-functions under the dual categories, Physics and Metaphysics,

giving the following:—*Thinking* as empirical and transcendental; *Perceiving* as individual and æsthetic; *Acting* as affirming and denying; the metaphysical exercise of these functions giving respectively, Philosophy, Art, Religion. (2) The principle of sin and evil which is egoistic affirmation. This is the root of both sin and suffering. (3) The Principle of Morality which is *denial*. Salvation from sin and suffering only comes through the denial of the will to life. (4) The way of achieving this self-denial of will. This includes, (*a*) the classification of springs of action arising from *Affirmation* and *Denial*; from the former *malice* and *egoism*, representing Paganisms; from the latter, *compassion* and *asceticism*, being the dominating motives of Christianity; (*b*) the two paths to self-denial, sympathy and suffering; (*c*) also the steps by which denial is achieved, justice, love, asceticism. (5) The goal of morality which may be expressed in various ways as the Kingdom of Heaven, Blessedness, Peace that passeth understanding, Nirvana. This state, as Deussen conceives it, is reached by a transcendence of individuality, but it is not purely negative. It is a state of positive experience and the denial itself cannot, therefore, be absolute.

Here, I think, we strike a crucial point in the metaphysics which Deussen represents. It seeks to make denial the last word in religious philosophy. But it manifestly is not the last word if the goal, the kingdom of heaven, nirvana, is not purely negative. In morality we strike a dualism between a lower and a higher self, as a basal fact. In the light of this, egoism becomes the affirmation of the lower self. But in its relation to the higher self it is denial and what the metaphysics which Deussen represents, calls denial, is in truth the *denial of denial* and is thus a higher affirmation. I do not see how the last word of morality and religion can be anything else than affirmation, an affirmation in which the highest self is realized.

From the psychological point of view Deussen's book possesses several points of interest. In common with the writers of his school he has done service to psychology in the emphasis he places upon the will. But just here, I think, we strike the greatest psychological defect of the school; its tendency to divorce too completely the intellect from the will. The inevitable result of this is a shallow conception of the intellect on the one hand and the identification of will with blind instinct, on the other. Between the two the teleological character of consciousness is lost sight of or inadequately treated. Again while Dr. Deussen's work is rich in fragments of psychological analysis, it is almost totally lacking in dynamic and genetic conceptions. Perhaps this is an unfair criticism to make on a work in met-

aphysics. But I think our whole metaphysical conception of the relation of phenomena to an absolute ground, or, in Kantian phrase, to things in themselves, will be profoundly affected by our psychological faith on this point. If we admit the genetic idea in psychology our whole world will become impenetrated with dynamism and it will no longer be possible to treat the phenomenal as mere appearance.

But enough of criticism. Dr. Deussen's metaphysics is one of the most valuable of the many works that have been appearing lately in English. And its English dress is in every way worthy of it and creditable to the translator. It is a book that no one can read seriously without getting rich suggestions and having his spiritual intuition greatly quickened. And the fine religious spirit that pervades it will commend it to every one who values the religious aspect of philosophy.

A. T. Ormond.
Princeton.

ETHICAL.

The Elements of Ethics. J. H. Hyslop. New York, Charles Scribner's Sons, 1895. Pp. VII., 476.

Dr. Hyslop's book might appropriately be entitled an analytic of Logical Conceptions. Its purpose is critical and analytic rather than constructive. The work has been done with that thoroughness and detail which we would expect from a man of Dr. Hyslop's ability and logical equipment.

After defining ethics and considering the sense in which it is a science, and its relations to other sciences, in his introduction, the author gives in Chapter II a very interesting digest of the history of ethical thought in ancient, mediæval and modern times down to Hume. This is valuable in itself and a very good Introduction to the discussions which follow. Chapter III is devoted to defining terms and stating and defining the elements entering into the ethical problem. In chapter IV on the Freedom of the Will, the most conspicuously able chapter in the book, Dr. Hyslop distinguishes the various species of freedom, identifies moral freedom with *velleity* or power of alternative choice, defends it against necessitarian objection and shows how man's physiological mechanism, through its function of inhibition, adapts him to the exercise of free choice and volition. The bearing of heredity and environment and the general question of motives are treated with ability and discrimination. Dr. Hyslop rejects indeterminism and identifies freedomism with the

species of determinism which recognizes the possibility of alternative choice. The discussion on freedom prepares the way for a discriminating analysis of Responsibility and Punishment in Chapter V. Chapters VI and VII treat of the Nature and Origin of Conscience. The analysis of conscience is interesting but need not delay us. In his chapter on the Origin of Conscience Dr. Hyslop discusses nativism and empiricism, and their various subdivisions. He is a nativist and yet gives generous recognition to empiricism in both its individualistic and evolutionary forms. Nativistic theories include three species, Theism, Naturalism and Intuitionism. Distinguishing two forms of Intuitionism, general and special, Dr. Hyslop accepts the former which affirms an original power distinguishing right and wrong, but not the right and wrong of particular acts, as the theory that is best borne out by the facts. This enables him to admit the claims of empiricism to a large extent and to recognize a wide sphere for evolution in developing morality out of its elements. The only concept of evolution which Dr. Hyslop rejects is that which ascribes to it a creative function and claims that morality can be developed out of conditions that contain none of it. From this point of view the theories of Darwin and Spencer are criticised and the position is combatted that the theory of evolution necessitates any radical reconstruction of ethical theory. In Chapter VIII, theories of the nature of morality are classified and discussed. Adopting a classification based on the *end* or *summum bonum*, Dr. Hyslop divides theories generally into Hedonism and Moralism. The Hedonistic theories agree in making pleasure or happiness the moral end and divide into Egoistic Hedonism and Utilitarianism. Moralism sets either excellence or duty as the end and this divides into Perfectionism and Formalism or, as some writers call it, Rigorism. A careful analysis of these conceptions of the end reveals elements of value in them all and supplies the data for a more adequate synthesis. The last two chapters, IX and X, discuss the important topics, Morality and Religion, and the Theory of Rights and Duties.

The merits of Dr. Hyslop's book are so great as to make criticism seem almost impertinent. I venture, however, to note several points on which I think a little more explicitness would be desirable. *First*, regarding freedom. Dr. Hyslop appears to limit the power of alternative choice to means. But is there not also a choice of ends, and is not this more of the essence of freedom than the mere choice of means? If our nature determines our end must it not supply dual alternatives for choice between higher and lower ends? Again, gradations of freedom are recognized and Dr. Hyslop

is willing to admit that some people may have very little of it. Does he mean velleity, and if so must he not recognize a more vital connection between heredity and freedom than he seems willing to admit. *Secondly*, it seems to me, and will perhaps strike others in the same way, that Dr. Hyslop has carried the legitimate distinction between the questions of origin and validity so far as to lose sight, partially, of the vital bearing which theories of origin must have on questions of validity. In spite of the logical separation, our psychogeny will determine largely the complexion of our metaphysics. And in this connection it seems to me that evolution has a more vital relation to ethical theory than Dr. Hyslop allows to it in his discussion. *Lastly*, respecting the relation of morality and religion, while agreeing with Dr. Hyslop's major proposition that the validity of moral science is not to be staked on the acceptance of any religious postulate, and with nearly all that is said in connection with it, I still feel that the religious thinker will have some grounds, in view of the whole discussion, for thinking that the problem has not been treated with sufficient insight. He will be likely to think that religion has been pushed a little too much to one side, and that just as it is possible to recognize the full right and independence of science in the sphere of its own categories, and yet to subsume it under the categories of metaphysics, so in the case of religion it is possible to recognize the full right and independence of moral science, while at the same time subsuming it under the categories of religion. The religious thinker will be disposed to regard the relation as one rather of comprehension and harmony, than of exclusion and mutual conflict.

But the faults of Dr. Hyslop's book are few compared with its merits. I feel under a great debt of personal obligation to the author for his masterly and luminous analysis. For the task that Dr. Hyslop has performed there was great need in this country and the work has been thoroughly done. His book is the most notable of recent contributions to the science, and will give him a front rank among ethical thinkers. It will also tend to raise the plane of ethical discussion in this country, and to put the problems of morality in a position where they can be more sharply defined and more intelligently treated.

A. T. Ormond.
Princeton.

Johnson's Universal Cyclopædia. Vols. I-V (A—Mozambique). New York, Johnson Co., 1894.

The striking feature of the philosophic content of this Cyclopædia compared, not simply with former editions of itself, but also with

other cyclopædias, is the much more adequate attention given to psychological topics. This may not unreasonably be attributed, I suppose, to the presence among its editors of Professor Baldwin; just as the editorial care of Dr. Harris had previously made the metaphysical side of philosophy more prominent in Johnson's than in any other save the Britannica. There seems to be particularly good reason for ascribing the difference to the interest of Prof. Baldwin in the fact that it is only in the fourth and fifth volumes, after Prof. Baldwin is well installed in the editorial chair, that the psychological articles become numerous. Some of the psychological topics which are so unfortunate as to begin with *A* or *B*, are in quite striking contrast to the accuracy and fullness of the later articles. The article on Association of Ideas, for example, gives a fair descriptive statement, but is quite innocent of modern problems and methods, to say nothing of results. In contrast with the definitely experimental tone pervading the later articles, it is somewhat startling to read regarding association, that the search for a physiological solution is in vain, and to find the following proposition set forth as an explanation: "This wonderful power of the human mind is part of the perfection which it owes to the Great Being who is its author."

The letters *G*, *H* and *I* are fortunately very rich in psychological captions and the comparative barrenness of the earlier pages is more than made good. I know of no better way to give an idea of the variety of topics treated than to give a running list of the more important subjects: Genetic Psychology, Genius, Habit, Hedonism, Hypnotism, Ideal Feelings (Emotion), Ideals, Illusion, Imagination, Imitation, Impulse, Innervation, Instinct, and Insanity, all by Professor Baldwin; Generalization, Hegel, Hindu Philosophy, Idea, Idealism, Identity, Immortality, Infinite, by Dr. Harris, and Intuitionalism, by the present writer. The article on Histology by Dr. Piersol should also be mentioned. In general, it may be noted that the neurological side is quite carefully looked after.

To go into as much detail regarding all the letters would render this notice a catalogue, not a review, but the articles on Localization (in space and of brain functions) and upon Motive by Professor Baldwin, and that by Dr. Cattell upon memory should be noted. It is in no way invidious to any of the other articles to say that the article upon memory is in respect to its objectivity, lucidity and presentation of current scientific problems and method, a model of what cyclopædia information should be.

Several of Professor Baldwin's articles seem to me a distinct advance upon his own statement of the same subject in his Psychology.

The idea is more definitely put, and the style more precise. There are many of these articles to which not only the 'general reader,' but the psychological specialist will turn with interest, and, judging from my own case (if I may venture for the nonce to pose as a specialist) with profit. The article, for example, on impulse is highly suggestive ; the reference of impulse to the central apparatus as representing the growth of the whole system, rather than to a specific stimulus, appears to be a very decided advance upon previous efforts to discriminate impulse from reflex-action. The article upon imitation is excellent, as we should expect from one who has made the psychology of that subject peculiarly his own. The article upon emotion (under the caption of Ideal Feeling) is admirable, save the attempt to state the theories offered in explanation. Of course not everything can be given in such an account ; and yet surely the contribution of James-Lange is too important, whether accepted or rejected, to be so briefly summed up. The attempt of Darwin to explain emotional expressions might well have received some attention. The article on Imagination would have been helped by reference to the concrete investigations in imagery; but aside from that it is well done. (There is a heading Generic Image, referring one to image, but the latter does not appear as a distinct topic ; it may also be noted in this connection that a *q. v.* to Insistent Ideas is found in the article upon Illusion, but no such caption occurs.) The article upon Genetic Psychology is too short to give Prof. Baldwin a fair opportunity, but fortunately we shall soon have a chance to read a fuller expression of his views. This present account is clear and full within its limits. But I wonder when I read the following : "Suppose we say, with many psychologists, that volition is necessary to all adaptive muscular efforts ; an appeal to the child shows us so many facts to the contrary that we are able to bring genetic psychology to refute the position." I do not wonder at Prof. Baldwin's saying this ; on the contrary, it is true enough to immediate facts. But I wonder if the final outcome of the appeal to the child will not be to change the ready-made concept of volition which serves as the standard in the above instance, and to generalize the idea of volition by making it equivalent to all acquired coördination.

However, I might go on indefinitely commenting upon points of interest. I shall fulfill my duty better if I divert the attention both of psychologists and the general public to the unusually full and suggestive discussion of psychological topics to be found in this last edition of Johnson's cyclopædia. Teachers will find its great value for reference further increased by the generally good and up-to-date bibliographies.

J. D.

EVOLUTION AND BIOLOGY.

From the Greeks to Darwin. H. F. Osborn. Columbia University Biological Series, No. 1. New York and London. Macmillan, 1894. Pp. X+259.

Amphioxas and the Ancestry of the Vertebrates. A. Willey. Columbia University Biological Series, No. 2. New York and London. Macmillan, 1894. Pp. XIV+316.

In these two volumes we have the beginning of a biological series which promises to be of importance for psychologists, since the topics of these and other volumes announced are the broader and more philosophical ones in the settlement of which the theory of the mental life is also involved. Prof. Osborn traces the history of the evolution idea before Darwin in an interesting way and with great perspicuity of style. From the psychologist's point of view more reference to mental development might possibly have been made; and yet it may be that the author found that his intimations of Darwinism before Darwin were not capable of such a reference. The book of Dr. Willey deals with the very vital question of the ancestry of the vertebrates; and while the conclusions upon the broader matters of descent are not large, still psychologists should know many more facts than they do of just the sifted kind which are here given. Our space only allows us to recommend these books, not on our own authority indeed, but as already approved by the biological authorities to whom we must defer.

The Factors in Organic Evolution. D. S. Jordan. Boston, Ginn & Co., 1894. Pp. V+149.

It is difficult to see what purpose this volume can serve. Dr. Jordan prints a great mass of catch-sentences, clauses, and words under the main headings of current evolution thought, sometimes calling upon his colleagues to treat special topics in the same brief and unsuggestive way. It is possible, of course, that the author may find such a 'syllabus' useful in the hands of his classes, while he himself fills out the outline by lecturing. But why he should publish it—why? Those readers who know what the terms and catch-words mean, and know intelligently, do not need to be reminded of the categories of the subject; and those who do not, are not taught. Possibly a few teachers who lack time to plan their own lectures may follow the author's skeleton. The peculiar way of printing the book with double blank pages throughout would seem to indicate that this is the writer's idea. But time-limits and sense-limits in different schools and colleges are so different that independent men will probably prefer to do their own schematization.

Illustrated Dictionary of Medicine, Biology, and Allied Sciences. G. M. Gould. Philadelphia, Blakiston, 1894. Pp. XV+1633.

The 'Dictionary' falls midway between two others which psychologists, who can afford them all, ought to have: One is Tuke's *Dictionary of Psychological Medicine*, and the other is Quain's *Medical Dictionary* (2d ed. 1894). Gould's book is more properly a dictionary, while the others are more properly encyclopedias. As a dictionary, Gould's work defines, in a reliable way, the terms of the whole group of cognate branches which touch upon biology and ought to serve a very useful purpose to psychologists, especially in these days when pathology, on one side, and development on the other, are bringing medicine and biology into such close touch with our own proper study. The present reviewer is not competent to criticise the definitions except as they are in his field; but judging from the sample topics in which he feels at home, and from the auspices under which the work appears, it seems altogether reliable. It is a pity more terms were not drawn from psychology for the benefit of the physicians and biologists who will be the main buyers; for they need instruction in general psychology.

Epitome of the Synthetic Philosophy. F. H. Collins. London, Williams and Norgate, 1894. 3d ed. Pp. XIX+639.

The original 'epitome' by Mr. Collins is now so well known that we need only call attention to this late edition. Its advance on the earlier editions is of course apparent, since it includes Mr. Spencer's last publications. The compiler indicates just what the addition is in these words: "By Mr. Spencer's kind permission I am enabled to include in this edition an abridgment to one-tenth (the proportion which holds all the way through) of his recent *Principles of Ethics*. The present volume thus represents in miniature the whole of *The Synthetic Philosophy* at present published."

J. M. B.

CHILD PSYCHOLOGY.

Notes on the Development of a Child. M. W. Shinn. Part II. University of California Studies. Vol. I, Berkeley, 1894. Pp. 89–178.

This is the second installment of Miss Shinn's valuable observations on her little niece. The paging is continuous through the two parts, making a total so far of 178 large pamphlet pages. The notes on the sense of sight are brought to a close by a chapter on 'Sight in the Third Year,' then Hearing, the Dermal Senses, Taste

and Smell are treated. By the end of the second year the discrimination of colors was practically complete, but no æsthetic interest in color-arrangements was apparent even in the third year. The child seemed, however, to be conscious of the defects in her attempts at drawing. Sensitiveness to sounds seemed different at different times, even apart from the effects of fatigue. Her attention was attracted by the experience of 'double touch' (the hands touching each other) on the 64th day. Sensibility to pain through hurts or extremes of temperature was very low during the first two months. "Taste at no time played as large a part among the child's interests," as the author expected. "To see others eating a favorite food was often desired as a substitute for eating it herself." If this comparative indifference to tastes was due (as the author seems to think) largely to training, it seems to me that a point is suggested here of the very highest pedagogical importance.

De la suggestibilité naturelle chez les enfants. A. Binet and V. Henri. Revue Philosophique, Oct., 1894. P. 338.

Understanding by 'natural suggestion' that form of influence which, in ordinary conditions, people exercise upon one another, the problem was to investigate the effects of natural suggestion upon the simple judgments of children. The children were graded according to age, as follows: 1st grade, children between seven and nine; 2d grade, children between nine and eleven; 3d grade, children between eleven and thirteen. The tests involved suggestions of three sorts:

(I) *The suggestion of a preconceived idea.*—Three lines of different lengths were shown to the child in succession; then, after a short interval, a chart was presented to him containing lines varying regularly in length, of which Nos. 5, 11 and 18 corresponded in length to the three models; and he was asked to pick out those lines which were equal to the models. Then the test was repeated with this difference: that in the chart now presented to the child the five longest lines (Nos. 17–21), and therefore the line corresponding to the third model were lacking. The force of suggestion would be felt in the expectation, on the child's part, of a uniform experience in the two tests (especially as in the second case the third model was shown him, just as in the first), and in his natural timidity and reluctance to declare the absence of the looked-for line, even if he suspected it. Over against this we must place the accuracy of his judgment and the correctness of his memory. The result was that a certain number believed themselves to find in the second chart a line of the length of the third model, and chose accordingly. Now

of these a certain number had in the first test chosen for the third model a line below No. 16 on the chart of 21 lines. These were now counted out, for the obvious reason that since they had made this erroneous judgment, where there was no suggestion, they could not be presumed to have made it *through* suggestion in the second case. The whole experiment was now repeated with the rest, with the result that the susceptibility to suggestion *was, on the average, in inverse proportion to the child's age.* Of the children in the first grade, 88 per cent., in the second 60 per cent., and in the third 47 per cent. yielded to the suggestion of the preconceived idea and selected a line for the third model from the second or incomplete chart. So far we have only judgments based upon memory. Now the child was allowed to see the model and the chart at the same time (direct comparison). Using only one model (the third) the results were: with the complete chart 67 per cent. of the children erred in direct comparison and 79 per cent. in memory comparison. When the 16-line chart was used, 38 per cent. made errors in direct comparison and 65 per cent. in memory comparison. There seemed also, in the case of direct comparison, more assurance and less timidity, and so less susceptibility to suggestion, than in the other case.

(11) *Verbal Suggestion.*—A line 40 mm. long was shown to the child, and he was asked to choose a line of that length from a chart as before. At the moment of his doing so, however, the experimenter said, in a calm, even voice, and without gesture: 'Are you quite sure you are right?' The result was that, in the case of memory-comparison, 89 per cent. of the 1st grade children, 80 per cent. of the 2nd grade, and 54 per cent. of the 3rd grade hesitated and then *changed their selection,* under the influence of the verbal suggestion. In the case of direct comparison the figures are: 1st grade, 74 per cent.; 2nd grade, 73 per cent.; 3rd grade, 48 per cent. Here, again, the younger the child the less stable his judgment and the more open he is to suggestion. Here, too, we see as before that fewer errors are made in direct comparison than in memory-comparison. Again it was observed that those whose judgments were correct were less open to suggestion than the others. Only 56 per cent. of the former to 88 per cent. of the latter changed their selections. Again, of those who changed their selection on account of the suggestion, 81 per cent. changed it *for the better* (*i. e.*, for one more nearly correct), only 19 per cent. changed for the worse. This is surprising, especially in the case of memory-comparisons: one would expect that the interruption would have made the child nervous, and so hindered instead of helping his judgment.

(III) *Suggestion in collective experiments.*—Taking four pupils at a time, they are allowed to see the model line and the chart of lines simultaneously, and then are asked to say, all at once, which line is equal to the model. Generally they do not all answer together, and the slow ones come under the suggestion of the quicker ones. In such cases there is a surprising uniformity among the younger children, the older pupils being more independent in their answers. Yet in all the grades there is a great susceptibility to this sort of suggestion. The percentage of correct answers was somewhat higher, when taken in this collective way, than when taken individually (34 per cent. to 23 per cent.).

A Preliminary Study of Motor Ability. J. A. Hancock, Pedagogical Seminary, Oct., 1894.

This study by Mr. Hancock well deserves the careful study of all interested in child education. A large number of school children from five to seven years of age were tested in various ways to discover the amount of muscular control they possessed. For full description of the tests and apparatus and detailed statements of the results the original article must be consulted. The results can be stated here only partially and in a general way, *e. g.* :

I. A child cannot stand so still as a man. Men swayed, on the average, 3.5 cm. in the anterio-posterior direction and 2 cm. laterally. Boys of six years swayed 5.1 cm. by 4.3 cm.

II. A child cannot hold his hand so still as a man. Men moved their hands on the average .242 cm. by .752 cm.; boys of six moved theirs 1.191 cm. by 4.258 cm.

III. A child cannot hold his attention upon any subject so steadily as a man. Some of these experiments, which required sustained attention for one minute, could not be carried out in the case of the children.

IV. Control of the arm is far greater in men than in children. With the former the 'Trenograph' registered an average movement of .0975 cm.; with the latter of .396 cm.

V. The child cannot *tap* so rapidly as the adult. The rate for the child of 16 years is five times as great as for one of six years.

VI. The order of control is from fundamental to accessory muscles (*i. e.*, larger muscles come under control earlier than smaller). Fine, complicated movements are difficult for the child.

VII. The prolonged effort to keep quiet produces in children strong symptoms of nervous irritation.

VIII. "Generally the girl, at the same age, is steadier than the boy."

IX. "Children in normal healthy growth show a lack of coördination and control paralleled only by ataxic, choreic and paralytic patients." F. TRACY.

TORONTO UNIVERSITY.

NEUROLOGY.

On the Inadequacy of the Cellular Theory of Development and on the Early Development of Nerves, particularly the Third Nerve, and of the Sympathetic in Elasmobranchii. ADAM SEDGWICK. The Quarterly Journal of Microscopical Science, N. S., No. 145, November, 1894.

During the past decade the protest has been coming from many quarters and with increasing strength against certain crude notions which, as accessories, have attached themselves to the cell doctrine.

Our author points out the condition of affairs, and summarises the usual training in these dogmas with telling effect. He then proceeds to illustrate the darkness of this biological age by several examples. During his various studies, especially those on Peripatus, he has been struck by the fact that in many cases tissues refused to break up into cells or cell layers. "It would appear," he says, "that in Peripatus the cells of the adults in so far as they are distinct and sharply marked off structures, are not, as appears to be generally the case, present in the earliest embryonic stages, but are gradually evolved as development proceeds. In other words, the cell theory, if it implies that the adult cells are derived from embryonic cells which have been directly produced by the division of the ovicell, does not apply to the embryos of Peripatus." For further illustration Sedgwick then takes up the so-called mesenchyme tissue of Elasmobranch embryos; the origin of nerve trunks and the fate of the neural crest.

The ideas fundamental to the view urged by our author are set forth in what is stated concerning nerve cells. According to this hypothesis nerve fibres are present before the nuclei or cell bodies appear. The principal function of the neural crest, so far as it takes part in forming the nervous system, is to produce nuclei which ultimately attach themselves at various points to this reticulum from which the fibres are formed by condensation. The details need not be given, for it is at once evident how very widely such a view differs from the one current, and according to which the nervous system takes origin from a series of spherical cells which later produce

the fibres as outgrowths. This new explanation certainly deals with but a fraction of the facts. Most anatomists are ready to admit that in the matter of cell formation as in other life processes there are wide variations, all the way from distinctly marked cell elements as disconnected as blood corpuscles through forms incompletely separated, to those in which nuclei appear scattered in a poorly divided enclosing mass. That any one of these arrangements should be chosen as representing the 'whole truth' and the others 'reduced' to it is more likely to hamper than to enlarge knowledge. It is psychologically interesting that we glory in the thought of transformation and genetic evolution, yet dominated by the notion of types and the mistaken idea that profound conceptions must be capable of simple expression, in the same breath utter a partial hypothesis which is warranted complete, and thereupon proceed to 'whip in' the non-conforming facts. There is but one excuse for this, namely, that each hypothesis must be pushed in every direction in order to demonstrate its truth or falsity, but we rely on our colleagues to exercise good judgment in the process and not mislead us, for grave responsibilities attach even to the exercise of the scientific imagination.

A description of the cerebral convolutions of the Chimpanzee known as "Sally"; with notes on the convolutions of other Chimpanzees and of two Orangs. W. BENHAM. The Quarterly Journal of Microscopical Science, N. S., No. 145, November, 1894. Plates 7–11.

Chimpanzee brains differ among themselves and the zoölogists hint at two or even three species of this animal. The individual 'Sally' who lived eight years at the Zoölogical Gardens in London has been referred by Beddard to the species Troglodytes calvus. The brain from this case is in some respects unsimian in its conformation.

In the majority of cases the chimpanzee brain possesses an occipital operculum, a distinctly simian feature. This was quite absent in the case of Sally. Further the demarkation of the insula and the branches at the anterior end of the Sylvian fissure were more than usually evident. Thus this specimen serves to diminish in various important characters the differences in form between the brain of man and of the chimpanzee as generally described. The accompanying plates are excellent.

The two other chimpanzee brains most similar to that of 'Sally' have been described by Broca and by Müller respectively. Both the latter brains were from young males, the species not having been exactly recorded. Hence there is no positive evidence that this type

of brain is characteristic of the species *calvus*, but at the same time it is not to be correlated with either age or sex.

Amusie (*Musikalische Aphasie.*) J. G. EDGREN. Deutsche Zeitschrift für Nervenheilkunde, B. VI. H. 1–2. December, 1894.

The perception and expression of musical sounds and symbols can be shown to be quite parallel to that for the sounds and symbols of ordinary speech. Beyond the musical faculty comes gesture language, which is a form of expression even more general than music. It has occurred to Ballet to picture the three brain areas concerned, as three concentric circles, of which that representing verbal speech should be the smallest and that for the emotional gesture language, the largest; the musical faculty falling between the two. Against such a scheme there are many important objections; but it serves to emphasize the fact that in any one instance the anatomical bases for the reactions are not identical with those in the others, although several structures may be used in common. In the study of aphasia, the disturbance in the musical faculty has been neither generally tested nor recorded in detail. E. is able to find in the literature, which he summarizes with great skill, a number of cases of aphasia without amusia; another group in which they are combined, and a third group, in which the amusia in one form or another, is alone present.

The impulse to his study of this subject was a case of amusia in which both the clinical history and record of the autopsy were at hand, and in which the brain lesion in the left hemsiphere was a destruction of the anterior two thirds of the first temporal, and the anterior half of the second temporal gyrus, together with destruction in the right hemisphere of the middle and posterior portions of the first temporal gyrus, and the ventral edge of the inferior parietal lobule along the Sylvian fissure. Both lesions are shown in figures.

In general the author concludes that the musical faculty like that of speech can be disturbed by lesions of the brain. The different forms of amusia are comparable with the different forms of aphasia. These are clinically distinct, and while the analogous forms of aphasia and amusia may occur together, they are not necessarily associated. There appears also to be a distinct anatomical basis for the forms of amusia as contrasted with those for aphasia and for that form of amusia designated as note-deafness (his own case), there is some reason to locate the cortical centre in the first and second temporal gyri of the left hemisphere, somewhat in front of the region, injury to which causes word deafness.

Recherches microscopiche e sperimentali su gli effetti della Tiroidectomia. F. CAPOBIANCO. Internationale Monatschrift für Anatomie und Physiologie, Band XI-H., II and XII, 1894.

There is often a tendency to overlook the importance of nutritive conditions in modifying the reactions of the nervous system. It has been shown, however, that there exists a close connection between the thyroid body and the central nervous system. On the pathological side the various forms of goitre, dependent on changes in the gland, and associated with disturbance of the nervous functions, give still further support to this idea, and Foster remarks in his text-book of physiology that the senescence of the nervous system is probably involved in the early atrophy of the thymus.

The work of Capobianco touches two points; the general effect of the removal of this gland from dogs and rabbits, and the changes which at the same time occur in the nervous system. As a result of the total extirpation of the thyroid gland, dogs and rabbits always die within four weeks, the average life of the rabbits being longer than that of the dogs.

Histological examination of the nervous system, central and peripheral, showed distinct pathological changes in cell-bodies and in the fibres together with alterations in the blood-vessels. In dogs, the entire central system is involved, while in the rabbits it is the bulb which is most affected.

The nature of the histological changes is that of an atrophy of both cell-bodies and fibres, while in some cases the cell-bodies show a granular disintegration and extreme vacuolization. The plates illustrating these changes are very striking and one is led to speculate on how far slight variations in the activity of this gland may initiate in the central system of a normal person, such changes as are here to be seen in an exaggerated form. H. H. D.

UNIVERSITY OF CHICAGO.

HEARING.

Beiträge zur Lehre von der Klangwahrnehmung. L. HERMANN. Pflüger's Arch., LVI, 10, 11, 12. Pp. 467–499.

Phonophotographische Mittheilungen. V. *Die Curven der Consonanten.* L. HERMANN and FR. MATTHIAS. *Phonophotographische Untersuchungen.* VI. *Nachtrag zur Untersuchung der Vocalcurven.* L. HERMANN, FR. MATTHIAS and A. EHRHARDT. Pflüger's Archiv., LVIII, 5 and 6. Pp. 255–279.

A Study of the Sense of Equilibrium in Fishes. S. LEE. Part II. Journ. of Physiol., Vol. XVII, Nos. 3 and 4.

On the ground of his well-known experiments with the wave-siren, A. König questioned Helmholtz's conclusion that clangs which differ only in the phases of their components are identical for our hearing. Hermann points out that experiments on the wave-siren are themselves questionable, from the fact that on this instrument difference of phase may result in similar sounds from clangs which are fundamentaliy different in the order of their over-tones. Reversing the curve of a clang as given in a phonograph, either along the axis of abscissas or of ordinate, changes the phases of the components but not the sound.

To the resultant sounds which are already known to arise from a combination of tones, Hermann would add what he calls a *Mittel-ton.* This tone arises from the actual resultant vibrations impressed on the conducting medium by the components of a clang. In the case of two component tones of the rates of vibration m:n, the number of vibrations of this 'median tone' within a beat-period would be $\frac{m+n}{2(m-n)}$.

From experiments on toothed disks, in which the phase of a tone was rapidly renewed, Hermann concludes that change of phase itself produces tones which may be and probably are the Tarturian tones. The Tarturian tone, therefore, is the intermittent tone from the medial tone. The article concludes with certain hypothetical additions to Helmholtz's resonance theory of tone sensations required to explain intermittent and beat tones. As regards simplicity, however, it is better to assume the direct excitability of the auditory nerve than to add a new series of epicyles to the resonance theory.

In getting consonant curves, Hermann found it necessary to multiply the motion of the phonograph, firstly, by an additional lever, secondly, by a ray of light reflected from the second lever. The motions of this ray of light were photographed. The entire 'plant' of apparatus is extremely complicated and delicate. The present communication gives the curves for *L.*

Applying the above apparatus to vowel sounds, Hermann obtained curves five (5) centimeters high. The curves confirmed his views published in former numbers of the *Archiv*, in regard to the fixed form of the characteristic vowel tone.

Dr. Lee's article is in continuation of his study on equilibrium published in Vol. XV. of the *Journal of Physiology.*

The chief find in the present article is that by stimulating the auditory nerve of the common dog-fish (galmus canis), the resulting

movements of the eyes and fins is the algebraic sum of the movements which arise from eliminating the ampullae branches separately.

STANFORD UNIVERSITY. F. ANGELL.

ATTENTION AND MEMORY.

Recherches sur la mémoire affective. TH. RIBOT. Revue Philosophique, XXXVIII, 376–401.

Affective Attention. E. B. TITCHENER. Philos. Rev., III, 429–33.

Affective Memory. E. B. TITCHENER. Philos. Rev., IV, 65–76.

M. Ribot attempts to show that pleasures, pains and emotions, as well as olfactory, gustatory and organic sensations generally, are not merely rememberable in an intellectual way as facts that have been experienced, but that they may themselves, in certain cases, be imaginatively reproduced. His conclusion is based on answers to questions received from some sixty persons, a goodly number of whom profess ability to revive, with varying precision and vividness, states of feeling in the manner indicated. Mr. Titchener disputes the interpretation. He does not deny that the recollection of exhilarating sport may be pleasant or that of a whipping painful, that a tooth may be made to ache again by suggestion of the former torment, or that the memory of an insult may excite anger. What he denies in all such cases—and it is on such cases that M. Ribot supports his contention—is that the affection thus aroused is itself a revival of the original affection, and not rather a real affection giving tone to or a real emotion prompted by an ideal object. Seizing on the implied admission that 'revived' affection always attaches to an ideational content, he urges that in that case there is no proof of revival of affection at all; it is a question, not of reproduction versus production, but of production by this stimulus rather than by that. It might be replied that the same is true of every sensational experience that imagination represents. The 'reproduction' is in fact a production, a new reality brought about by the action of a stimulus on a disposition to function, attached by association to elements other than those which attention emphasizes after the recall, and only identical with the original experience as being the same in kind. But the objection lies deeper. The real question, as Mr. Titchener conceives it, is whether pain, pleasure and emotion are possible objects of attention at all. It is because he believes that they are not—a point argued in the earlier article—that he holds that they cannot be singled out and identified in imagination. What can be attended to are the sense-contents. These, however, are condi-

tions, not constituents, of the affectional element, and the denial of our ability to imagine the latter as distinct from really experiencing it as present felt qualification of the represented content appears to the author highly important as a matter of psychological principle.

Certainly, as a matter of pure introspection, it would seem impossible for the attention to fasten on any content corresponding to the abstractions 'pleasure-pain' or 'psychic attitude.' If, as Mr. Titchener maintains, a feeling is 'properly analyzed into sense-substrate and affection,' nothing can be discovered among the objects of direct consciousness corresponding to the latter. On the other hand we can attend, as Mr. Titchener allows, to our concrete feelings. A toothache, a state of grief or terror, can be as distinctly felt as a patch of red color or a movement in the joints. And if felt, then represented as felt, with something, no doubt, of the repercussion of the original excitement. Without this the object represented is not really the same, and the experience is remembered much as a color is remembered which is not visualized; we know, that is, its name, perhaps some of its concomitants. Apart from these experiences, there is nothing in pleasure, pain and emotion for psychology to deal with: they are mere names which express, not psychological experience, but the practical value of the experiences which they qualify.

SMITH COLLEGE. H. N. GARDINER.

REACTION-TIME.

Zur Beurtheilung der zusammengesetzten Reactionen. W. WUNDT. Philos. Stud., X, 485–498. 1894.

Beobachtungen bei zusammengesetzten Reactionen. Zwei briefliche Mittheilungen an den Herausgeber. E. KRAEPELIN und JULIUS MERKEL. Philos. Stud., X, 499–506. 1894.

Simple Reactions. E. B. TITCHENER. Mind, N. S., 13, 74–81. Jan., 1895.

Two Points in Reaction-time Experimentation. R. WATANABE. Am. Journ. of Psychol., VI, 408–512. June, 1894.

The articles in the *Philosophische Studien* call attention to an aspect of experimental psychology sometimes overlooked — namely, the importance of the knowledge that may be derived from introspection in the course of psychological experiments. Thus Prof. Wundt states explicitly that his theory of the development of the will, and of its relation to 'apperception,' had its origin in observations made during the course of experiments on reaction-time. He

concludes his discussion by saying that the times measured have only an incidental interest—the real value of such experiments lies in the fact that they subject mental processes to fixed conditions, and thus make possible an exact analysis by introspection. While much can be said for this point of view, the present articles do not give conclusive testimony in its favor, as they are controversial, the introspective evidence of some observers contradicting that of others.

Wundt argues for the interpretation of sensory and motor reactions, 'perception-times,' 'discrimination-times,' 'choice-times,' and 'association-times' already given in detail in the fourth edition of the *Physiologische Psychologie*. Wundt is regarded as the great representative of the experimental and scientific method in psychology—and deservedly so—but he does not readily adapt himself to the scientific attitude that weighs evidence and waits for evidence. He considers it possible and desirable to pass final judgment on every question great and small. This he does with much learning and ability, but often without proper perspective. He sees the world as a panorama with himself in the centre. He forgets that a panorama, constructed from fragmentary data, holds only for the individual who constructs it—also that there is no centre of infinite space.

The experiments on sensory and motor reactions do not seem to the present writer nearly so important as they do to Wundt, nor can he admit Wundt's interpretation of the facts. When Wundt informs us that "zu Versuchen über den zeitlichen Verlauf psychischer Vorgänge ist nun von vornherein nur ein Beobachter fähig, der im stande ist, willkürlich zwischen diesen beiden Reactionsformen zu wechseln," he is proposing an esoteric psychology, not a scientific method. Wundt insists that the 'subject' in psychological measurements must always be a skilled psychologist. Yet he writes on *Thierseele!* The investigator should, indeed, be a skilled psychologist, able to interpret the facts, but a psychologist with a theory to prove is not a good observer.

Every one who wishes to make psycho-physical time-measurements should read the article by Wundt and the letters in the same number of the *Studien* by Prof. Kraepelin and Dr. Merkel—not in order to accept as a matter of course the observations given—but in order to realize the need of observing and recording the changes in consciousness accompanying such experiments.

Prof. Titchener's discussion of sensory and motor reactions in *Mind* is more careful and judicial than is Wundt's. He sums up the

evidence of ten researches and finds six favorable to the distinction and four more or less negative. Prof. Baldwin, however, seems to be counted on the wrong side, as he finds (in a publication later than the one quoted) the nature of the difference to vary with the observer; and Prof. Titchener himself has found the distinction in less than half the cases he has tested. We may conclude that the normal reaction-time of an observer can often be lengthened by directing him to fix his attention on the sense-impression, but it does not seem so evident that it can be shortened by directing him to fix his attention on the movement. The reaction-time is naturally lengthened and made more irregular when its automatic nature is disturbed; and from the experiments made in the Leipzig laboratory, it would seem that attending exclusively to the sense-impression is more disturbing than attending exclusively to the movement. In daily life, however, the contrary holds; actions are executed more automatically when the attention is directed to the sense-impression—thus in throwing, catching or striking a ball, the more completely one can attend to the ball and forget the movement, the more efficient and quick is the movement. Indeed, in reaction-time experiments, when the stimulus is so strong as to compel the attention (as with painful electric shocks), the reaction-time is very short, which would seem conclusive against the extreme views of Lange and Wundt. That the difference between the times of sensory and motor reactions gives the time required to perceive the stimulus (Wundt and also Titchener in his earlier paper, *Philos. Stud.*, VIII.), does not seem admissible to the present writer.

In the short paper by Prof. Titchener and Mr. Watanabe, attention is again called to the desirability of treating reaction-time experiments from the point of view of *psychology*. The observer's impression regarding the nature of the reaction is recorded. The writers conclude that in the case of sensory reactions introspection affords an adequate control, but is less trustworthy in the case of muscular reactions. J. McK. C.

JUDGMENT AND BELIEF.

Glaube und Urtheil. W. Jerusalem. Vierteljahsschirft für wissenschaftliche Philosophie. Vol. XVII, pp. 162-195.

Grundzüge der Logik. T. Lipps. Hamburg u. Leipzig: Voss.

Principii di Logica Reale. N. R. D'Alfonso. Rome: 1894.

Appearance and Reality (*passim*). F. H. Bradley.

The Test of Belief. J. P. Gordy. Philosophical Review, May, 1894, 257.

Few states of consciousness, or psychoses, whether viewed from the psychological or from the epistemological standpoint, are

more interesting or more important than those indicated by the words 'judgment' and 'belief.' If it be true that the whole essence of the thinking process is involved in the formation and expression of judgments; that judgment is not so much a mere occurrence in the mind as an activity *of* the mind; that the test of a genuine act of judgment is the presence in it of belief; and that in all judgment there is thus a 'trans-psychosial' reference, a reference, that is, to reality beyond the factual sphere of the psychosis as such:—if all this and much more that we are told of judgment and belief, be true, then it would scarcely be too much to say that a good means of testing the psychological and even the epistemological position of any writer would be to ask "What is his doctrine of 'belief' and of 'judgment'?" We summarize a few recent utterances bearing upon these subjects:—

Herr Jerusalem calls attention to the fact that in recent times the view has often found expression that the essential characteristic of the act of judgment is the consciousness of its objective validity, called by the English *belief* and by some German psychologist *Anerkennung*. This view has been urged especially by J. S. Mill (Notes on Jas. Mill's *Analysis*, I, p. 342, and *Exam. of Ham. Philos.*, p. 405) and by Brentano (*Psychol. vom empir. Standpunkte*, I, pp. 269 f.). Attention is also called to the important discussions of belief by James (II, 282 ff) and Baldwin (II, ch. 7 and *Mind*, N. S., I, 403); the last named has handled in a very noteworthy manner the question of the relation of belief, feeling and judgment. The trans-psychosial reference inherent in every judgment and characterizing it as something more than a mere psychosis, a mere affection of consciousness, was recognized even by the ancients, *e. g.*, by Plato (*Theat.*, 184-187) and the Stoics (Cicero, *De Fato*, 19, 43). Descartes and Spinoza emphasized the presence of a conative element in judgment and in belief; with them judgment is predominantly an assent of the will, an affirmation. The history of the problem of judgment shows that it has been handled either in a one-sidedly *psychological* or in a one-sidedly *grammatico-logical* manner. Baldwin has rightly insisted that a complete theory of judgment can only be attained when all the constituent factors, or elements, entering into the act are given full recognition.

Herr Jerusalem thinks that the whole subject of judgment needs to be investigated anew, and especially does the relation of belief to judgment need to be made clear.

Judgment is, he finds, an activity by which the complex of sensation, or manifold of sense, is discriminated and combined, moulded

and articulated, and objectified, *i. e.*, regarded as an independent unitary being with powers. Consciousness in judging conceives the given manifold or complex as the activity of a thing. Judgment is essentially 'ein Gliedern und Gestalten.' An injection of an element of willing into the presentative complex is the most important factor in an act of judgment. In fact, in judgment the given content of sense—presentative content—is formed or moulded by a process analogous to the activities of our own will, and objectified or conceived as an activity or quality of a thing. In this objectification we find the germs out of which belief and the conception of truth later develop. This objectification being present implicitly in sense-perception, we may say that even in perceiving we judge.

What now is the relation of judgment to truth? Truth, as already said, is implicit in the objective reference characteristic of all judgment. Mill is right in saying that to judge and to regard the judgment as true are identical. This, at any rate, is true of original and naïve judgments. The full consciousness of truth is, however, only reached when by experience we are taught the possibility of error. The truth of a judgment is the relation between the judgment as a psychological fact and the judged event. We denote this relation by word the 'accordance' (*Entsprechen*). The idea of truth first arises by reflecting on this relation. Such reflection, however, only becomes possible when we discover that wrong interpretations, mistakes, occur. In defending the meaning contained in a judgment against possible assaults the consciousness of truth emerges. The conception, therefore, of truth presupposes experience of error. Truth and error both belong properly to the sphere of judgment. Bradley's distinction between an 'idea as a fact' and an 'idea as a meaning,' more properly holds of judgments than of concepts. We can, that is, distinguish between a judgment as state of consciousness and a judgment as having a 'meaning'; and truth is the relation of these two sides of the judgment to each other. Indeed only in a system recognizing a world of extra-mental realities, independent of judgments and to which they may conform or not, is truth possible; that is, truth presupposes psychoses and a trans-psychosial world of realities; deny either and the merely factual, not truth, is all that is left. The criteria of truth are found in the fulfilment of predictions and the agreement with other thinkers.

What now is the nature of belief, and what is its relation to judgment? An element of belief is implicit in the act of judgment; but this embryonic belief is to be carefully distinguished from belief in the higher sense. Belief as a clearly experienced state of con-

sciousness is the holding as true of a judgment and therefore it presupposes judgment and the concept of truth. Yet the truth of a judgment is in no way a condition of belief; untrue judgments are believed as well as true ones. The English psychologists are right in finding in *feeling* the source and essence, psychologically, of belief. The opposite of belief is not disbelief but doubt, which is generally and rightly regarded as feeling; belief, therefore, is predominantly feeling. 'Predominantly,' for all psychic facts—all really experienced psychoses—consist, without exception, of more than a single factor, comprise always intellectual, conative, and affective elements. They are named and classified according to the predominant factor, and in the case of belief, this is *feeling*. Belief, as here used, is not to be contrasted with knowledge; it is used in the general sense of 'holding as true.' What calls forth this feeling which attaches itself to a judgment and turns it into one held as true? The answer is, that belief is the *feeling of harmony*, or agreement with the previous content of my consciousness; the feeling of the accordance of a judgment with my conceptions of the world. Just as doubt arises from the conflict of a judgment with my previous thoughts, so the feeling of belief springs from their harmony.

Herr Jerusalem's paper is a very meritorious one and will repay study.

Prof. Lipps also emphasizes the objective, or 'trans-psychosial,' reference in all judgment. Judgment is the consciousness of the objective necessity of a relation, or union, of the objects of consciousness. The logical doctrine that judgment states only what is true or false, is sound. Truth is synonymous with real knowledge. The distinction is made—fundamental for logic—between real and formal judgments. In a formal judgment the objective necessity is an unconditional necessity prevailing among notions; in a material judgment the objective necessity is that of relating, to an object of consciousness thought as objectively real and so far as it is thus thought, another also thought as objectively real. The objectively valid judgment is the special act of real knowledge. A judgment is objectively valid when the consciousness of the objective necessity perdures, without contradiction, against all possible experience and objectively necessary union of the objects of experience. Objectively valid judgments, hence knowledge, arise in the struggle and interaction of the proximate subjectively valid judgments. Every judgment is subjectively valid in so far as it is made.

The universal validity, or validity for all, follows from the objective validity, on the assumption of similarity in the thinking

processes of all thinking beings. That is, the claim to universal validity of a judgment lies in the conviction, that, on account of the similarity of all minds, all must reach like judgments, in so far as they have the same experiences and relate them by thought. Prof. Lipps' discussion is, from the logical standpoint, singularly fresh and helpful.

Signor D'Alfonso in his little treatise on concrete logic has some remarks of interest on judgment (*considerazioni sul giudizio*). All thinking and reasoning are essentially judging; in judgment is involved the whole of the thinking process. Every judgment implies in one and the same act a synthesis and an analysis; these are the two sides of every judgment. Every so-called negative judgment can be transformed into a positive one. When we assert that a given body is not solid we implicity assert that it is liquid or gaseous. Negation is, it would seem therefore, a judgment on a judgment and thus presupposes an affirmative judgment. Psychologically affirmation is prior to negation—in fact all judgments are, psychologically, affirmative. That is, as concrete mental processes there is no distinction between positive and negative judgments; the attitude of mind in up and down negation being the same as in affirmation. It is, therefore, the *non-licet* attitude of mind, the refusal to (logically) affirm or deny, which is psychologically the opposite of judgment.

Mr. Bradley is more interested in the epistemological and metaphysical aspects of judgment and belief than in the purely psychological. But his book is full of keen psychological analyses and deserves, as was made evident by Prof. Baldwin in a late number of this REVIEW, the attention of students of psychology. Those acquainted with Mr. Bradley's *Principles of Logic*, will not be surprised to find that in the more recent work he has a good deal to say of judgment. We extract a few pregnant statements: In judgment, according to Mr. Bradley, we find thought in its completed form. Judgment is the differentiation of a complex whole, and hence always is analysis and synthesis in one. It separates an element from, and restores it to, the concrete basis. And here obviously the synthesis effected is a re-union of the distinguished, and implies the separation, which, though it is over-ridden, is never unmade. The predicate is a content which has been made loose from its own immediate existence and is used in divorce from that unity.

In every judgment there is in the subject an aspect of existence which is absent from the bare predicate. No one ever *means* to assert about anything but reality, or to do anything but qualify a

'that' by a 'what.' Judgment adds an adjective to reality. In every judgment the genuine subject is reality, which goes beyond the predicate and of which the predicate is an adjective. The predicate, on the other hand, is a mere 'what,' a mere feature of content, which is used to qualify the 'that' of the subject. In every judgment, then, we find an aspect of existence, absent from the predicate but present in the subject, and in the synthesis of these aspects we have got the essence of judgment.

Prof. Gordy's paper is a fitting companion-piece to Herr Jerusalem's. It is so full of matter that it is difficult to condense it and yet do it justice.

The distinction between the pure intellect seeing and the practical intellect trusting, or between knowledge and belief, Prof. Gordy considers of fundamental importance. Belief of any kind consists, he declares, of two factors: what, with Baldwin, we may call the reality-feeling, plus the 'consciousness of the personal indorsement of reality.' One of these elements or constituents of belief—the reality-feeliug—we may have without the other—the personal indorsement of the reality; the saying to one's self that the reality-feeling is true. "Sitting in a car at a depot, waiting for my train to start, I seem to see the motion of my train when another train moves slowly by. In other words, the reality feeling attaches itself to the image or idea of my train in motion. But when I look at the wheels of the moving train this reality feeling ceases to exist so long as I continue to look at them. I see or believe that the apparent motion of my train is due to the real motion of the other. The same kind of reality-feeling attaches itself to a new set of experiences. But as soon as I stop looking at the wheels, the old reality-feeling returns—my train seems to move in spite of the fact that I know it does not. In other words, the reality-feeling, which alone distinguishes the ideas or images of memory from mere imagination, attaches itself to experiences which we know from other evidence do not represent reality." For further discussion of this point we are referred to 'Baldwin's able and very lucid treatment of the subject,' *Feeling and Will*, ch. 7.

By a critical examination, containing much that is suggestive, of Prof. Bain's three postulates or assumptions underlying all material or inductive inferences, and of J. S. Mill's theory of induction, Prof. Gordy reaches the conclusion that in order to carry on the reasonings of ordinary life as well as those of science, we must assume (1) the trustworthiness of memory within certain limits, (2) the uniformity of nature, and (3) that an hypothesis that explains a particular group of facts, and at the same time harmonizes with the rest of our

beliefs, is true. We can give no reasons for such beliefs which would at all satisfy a cold, critical intellect, an intellect indifferent to consequences, an intellect that believes only in so far as it sees grounds for certainty or for probability. Now from the point of view of the pure intellect, the intellect *seeing*, not trusting, these beliefs have neither certainty nor probability; from the point of view of the practical intellect, the intellect yielding to the native instincts and unreasoned tendencies of the mind, they are not only probable but certain. From the point of view of *knowledge*, in a word, our beliefs are so many pure assumptions.

Now we need a test of belief. By 'test of belief,' Prof. Gordy does not mean a test by means of which we can determine the truth of our beliefs, that would be a test of truth. He means a formulation of the marks or characteristics of the beliefs that we are obliged to assume without proof. Now we can say that, since we have accepted the trustworthiness of memory and the uniformity of nature and the proposition, 'an hypothesis that explains facts, and at the same time fits in with everything else that we believe is true,' we will accept any other proposition without further proof that has the same characteristics. What, then, are the characteristics of these beliefs? The assumption of the trustworthiness of memory has two: (1) it is a belief that we have a natural tendency to make,—*i. e.*, when we begin to reflect we find ourselves making it;—and (2) experience does not deprive us of it. The second characteristic—the confirmation of experience—must be taken in a negative sense only. Of positive verification of the trustworthiness of memory, we have none. The thesis which Prof. Gordy maintains, then, with reference to the trustworthiness of memory is this: What we know on the authority of what we call memory has no other guarantee than a reality-feeling, a feeling which sometimes attaches itself to experiences that we know do not represent realities, but which we accept in the case of memory, simply because it is *not contradicted by other experiences*.

The characteristics of our belief in the uniformity of nature are the same; we have a natural tendency to make it, and our experience is not inconsistent with it. What again, are the characteristics of the third assumption: An hypothesis is true that explains the facts, and that takes its place easily and naturally among our other beliefs. They are the same. These, then, are the characteristics of the three assumptions (beliefs), one of which underlies all reasoning whatever, and all of which underlie the reasoning of inductive science and everyday life.

Necessary truth, then, aside—truth, that is, whose contradictories are 'absurd, inconceivable, impossible,'—whatever we are asked to believe, ought to be *either* an ultimate belief, *i. e.*, a belief having the characteristics of being assumed through a natural tendency, and of not being interfered with by experience, *or* an hypothesis that explains all the pertinent facts, and takes its place easily and naturally among our other beliefs. The broader the base of experience upon which beliefs, in the negative sense explained, rest, the greater their credibility. If one man accepts one hypothesis because it explains all the facts he knows, and another man a different hypothesis because it explains, not only the facts known to the first man, but others equally certain, the last man's hypothesis is the more credible, although we can never say that it, in turn, may not have to give place to another.

Such a theory, it may be urged, opens the door to unbounded credulity. Not so, says Prof. Gordy, for the very prominence which it gives to the fact that inductive reasoning is only a process of finding hypotheses to explain facts, cannot but enforce the necessity of caution on the part of one who accepts it. Again, it may be urged, that its practical outcome is philosophical skepticism. Not so, for he only can be charged with philosophical skepticism who holds that reason is hopelessly at war with itself; who holds that, no matter upon what subject or in what direction he tries his reason, it leads him into an inextricable tangle of inconsistencies and contradictions. With the common-sense philosophy, the theory insists that the attempt of the empiricist to find positive verification in experience for the first principles of science cannot succeed; with empiricism, it insists that the attempt of the common-sense philosophy to establish definite philosophical principles must end in failure. Finally, the theory aims to give full recognition to the important, nay, the decisive, part which the emotional and volitional side of our natures play in shaping our beliefs.

YALE UNIVERSITY. G. M. DUNCAN.

PATHOLOGICAL.

Psychiatrie. TH. ZIEHEN. Berlin, Friedrich Wreden, 1894. Pp. 470.

The author has already made several contributions to physiological psychology and the present text-book on psychiatry is frankly written on psychological lines, as distinguished from clinical. Ziehen claims that the association psychology is entirely sufficient to explain all the facts of psychiatry and over one third of the book is given up

to general psychology, and he discusses the disturbances of sensation, of ideation and memory, of the intellectual feelings, of the association of ideas, of behaviour, and the accompanying somatic symptoms of the psychoses. The psychology is orthodox, albeit somewhat dry, but is on the whole satisfactory, and furnishes a good compendium of the perverted mental operations of mental disease. In his classification Ziehen makes but two grand subdivisions, psychoses without defect of intelligence and those with such defect. Under the first coming the simple psychoses, mania, melancholia, neurasthenia, stupor and paranoia, and the combined psychoses—the insanities secondary to the above. Of the psychoses with defect of intelligence there are first the states of congenital defect, idiocy, imbecility and debility, and secondly the psychoses from acquired defects, the six forms of dementia, paralytic, senile, secondary after brain lesions, secondary after functional psychoses, epileptic and alcoholic. In this simplification of classification there are several important omissions. *Delirium acutum* is denied a place as a distinct clinical entity, against the opinion of the best alienists, and is only spoken of as occurring in acute hallucinatory paranoia and in general paralysis. Again, periodical and circular insanity are simply assigned places as varieties of mania and melancholia, and under the degenerative psychoses. But it is in the field of Paranoia that recent German writers have especially run riot, and Ziehen adds greatly to the already existing confusion. The moment we depart from Krafft-Ebing's definition that paranoia is a chronic disease, showing itself exclusively in degenerate individuals, and frequently developing from the constitutional neuroses, and whose chief symptoms are [systematized] delusions,—we are landed in inextricable confusion. Ziehen's acute hallucinatory paranoia terminating in recovery without mental defect in over 70 per cent. of all cases, is not 'Verrücktheit,' and bracketing them together only tends to confusion and false ideas. Ziehen makes four forms of paranoia, the acute and chronic hallucinatory, and the acute and chronic simple paranoia. In his etiological summary of the psychoses the following etiological factors are credited with producing, besides many other psychoses, different forms of paranoia, as follows: Hereditary degeneration, three forms; trauma capitis, two forms; chronic alcoholism, five forms; puberty, three forms; senility, two forms; climacteric, two forms; puerperal state, two forms; lactation, two forms, acute febrile diseases, three forms [in reality confusional insanity]; epilepsy, two forms; hysteria, four forms; exhaustion, four forms. It would be hard to conceive of confusion worse confounded. Ziehen's simple chronic paranoia is the only one that fulfils the condition of a

chronic primary disease with systematized delusions. When it is considered that Jean Jacques Rousseau, Ludwig of Bavaria and Guiteau are classical examples of paranoia in its proper and more restricted sense one sees the folly of speaking of a man suffering from a blow on the head, or from pneumonia or multiple neuritis, where mental disturbance develops, as being a paranoiac.

Ziehen adopts the sound modern doctrine that in the immense majority of cases gynaecological treatment is entirely without influence, while in other cases by setting up new irritations it is positively harmful.

Katatonia is admitted to a position as a clinical entity, but is given as of rare occurrence.

The clinical descriptions are clear, and the ten photographs are remarkably successful in giving the physiognomy of the different diseases.

FOXBORO, MASS. WILLIAM NOYES.

Les états intellectuels dans la mélancolie. G. DUMAS. Paris, Alcan, 1895.

This little volume is worthy of more than casual notice. M. Dumas, a pupil of Ribot, belongs to the group of contemporary psychologists who commenced their study with philosophy and metaphysics, and then changing face, have gone over into medicine. MM. Janet and Marillier did the same.

This study on melancholy is Dumas' thesis in medicine. One can not praise too highly the courage of those who, while already having degrees and titles as doctors of literature and professors in schools and colleges, yet devote themselves to undergraduate work in medicine. Their intellectual experience is peculiar. Instead of commencing the study of psychology with observation and fact, they have approached it from the side of the more abstract and metaphysical problems. They seem to put the chariot before the horse; and it becomes an interesting question what attitude this leads them to bring to the empirical study of medicine. Do they still remain metaphysicians?

Dumas' study relates to four women of the asylums of St. Anne and Salpétrière in Paris, all afflicted with melancholy. He studies their mental state with their physical symptoms. He distinguishes four forms of the trouble: Melancholy with stupor, 'anxious' melancholy, 'depressive,' and 'conscious' melancholy. The author occupies himself mainly with the last two kinds.

His main method of study was by conversation with the patients, seeking to gain their confidence, questioning them of their griefs,

endeavoring to reason with them and to reassure them. Evidently this is a more fruitful method than direct experiment since the field of mental disturbance is so wide. Yet the author felt the need of more than this bird's-eye view of his patients' mental state and sought to study more exactly the rapidity of perception and memory, the naming of objects, and the localization of sounds, &c. His arrangements for this were a little inexact; for without careful arrangements for measuring intervals of fractions of a second no definite results can be obtained. The same may be said of the methods of studying physical symptoms: the author is satisfied with stating *what he saw:* such as attitudes, changes of cordiac pressure (from 800 gr. to 500 gr., on a Verdin sphygmometer, &c. He himself says that he might have given his results more exactness by plotting curves and giving tracings. Yet he concludes: "I am convinced, after many efforts, that the methods of psychophysics are not applicable to phenomena so complex as those I wish to study" (p. 142). But would it not have been better to publish his figures and tracings and then to show by a critical discussion of them why these exact methods are not applicable to these patients?

In the opinion of the author the ground of melancholy is not emotion, a psychological entity, but an organic state, a depression of the organism, a lack of nutrition. It is a predominant activity of the organs which produce the particular sensations contributing to the mental state of sadness, anxiety, depression. In this view he discusses the theories of James and Lange with just criticism. He explains from this point of view the infectious character of melancholy, citing the general vital depression which follows an attack of influenza. "It may be objected," says he, "that organic depression is produced some times as a consequence of mental trouble: the fact can not be denied, but it is far less frequent than we think." And he cites instances of the contrary (p. 100).

We need not say that the author is right here, only we should advise him, if he would go deeper, to make his distinctions more exact, *i. e.*, to show how physiological depression lies at the basis of melancholy. It seems probably true; but depression is a very vague word and the author finds depression present in a variety of diseases whose emotional tone is very different from one another, *i. e.*, hysteria, dyspepsia, heart troubles, &c. Has not the particular organ affected in each case, some special importance? Would it not be interesting to enquire into the particular organic derangement which is found in each of these troubles?

Although melancholy is, in the opinion of the author, consciousness of a state of the body, yet in certain instances it may

arise from intellectual conditions ; it may take its origin in an event which distresses, depresses, and finally enfeebles the patient. So also melancholy which arises from an organic cause is always aggravated by the distress which results from it, so that the psychological phenomenon, be it cause or effect, always plays an important part in the development of the disease. There is a series of complex actions and reactions between the physical and the mental.

The second question which the author studies is the nature of the intellectual changes which take place in melancholy. The principle features of this mental state, according to his short and summary descriptions, are 1. The slowing of the mental flow and great mental impoverishment ; 2. Aboulia, or the incapacity to carry out an act conceived. Of this the author cites two examples. One of his patients made careful preparations for suicide, but lacked courage at the critical moment ; another wished to write a letter but desisted from scruples of doubt. He explains it as a defect of idio-motor synthesis ; 3 The development of automatic acts sometimes very grave. One patient suddenly attempted suicide and had great trouble in recognizing herself as the perpetrator. This proves the act automatic. Indeed she thought the command came from a foreign will. In this connection the author studies the melancholy of Hamlet in whom he finds the signs which he thinks characteristic of melancholy; 4. The last mental sign of melancholy and the most important is the lack of logic: a patient weeps from organic causes merely and without knowing why, or when the necessity arises of thinking of some old distress long since forgotten in the past. She nurses the thought of these miseries and comes to believe that they cause the present grief.

In conclusion we may hope that the author will continue and deepen his study of these questions and give us more extended observations in detail. As it is his little book is clear and attractive.

Cliniques des maladies du systéme nerveux. J. M. CHARCOT. Tome II. Paris, Alcan, 1893. Pp. 482.

For some time M. Charcot, like the true teacher that he was, associated his students with all his labors, and even allowed them to take his place in some of his clinical lectures, to give them opportunities to explain, to all those who frequented the Salpétrière, the questions studied by him. It is in this way that after preparing a study on great calculators, in co-operation with M. Charcot, I was requested by him to deliver a lecture at the Salpétrière on memory for numbers. The present volume, prepared some months after the

death of the eminent professor, has been written with the co-operation of a great number of his pupils; only five or six of M. Charcot's own lectures are included in the volume. They bear on subjects that do not all equally concern psychology; but psychologists may read with profit the lecture on hysterical hemianaesthesia and toxic anaesthesia (p. 460), and with still more profit the one on retro-anterograde amnesia. Let us recall in a few works what this anesthesia consists in. The question is of a patient who after a nervous shock and crisis had retrograde amnesia, and also because incapable of registering actual facts in memory. M. Charcot shows clearly that this amnesia is not real but apparent. The patient remembers very well the facts which she seems to forget, because she talks in her sleep of facts of which she has no idea in her waking state; and moreover in the hypnotic state she remembers all the incidents of the whole period since the nervous shock. (p. 266). This volume also contains equally interesting studies by Guinon and Blocq on states of somnambulism.

SORBONNE, PARIS. A. B.

NEW BOOKS.

The Philosophy of Mind. G. T. LADD. New York, Scribners, 1895. Pp. XIV + 414. $3.

Elements of Ethics. J. H. HYSLOP. New York, Scribners, 1895. Pp. VII + 470. $2.50

Monism as Connecting Religion and Science. E. HAECKEL. London, Black; New York, Macmillan, 1894. Pp. VIII + 117.

The Factors in Organic Evolution. D. S. JORDON. Boston, Ginn & Co., 1894. Pp. V + 149.

Comte, Mill and Spencer: An Outline of Philosophy. J. WATSON. Glasgow, Maclehose; New York, Macmillan, 1895. Pp. XX + 302. $1.75.

Mental Development in the Child and the Race: Methods and Processes. J. M. BALDWIN. New York and London, Macmillan, 1895. Pp. XVII + 496.

Amphioxus and the Ancestry of the Vertebrates. A. WILLEY. Columbia University Biological Series, II. New York and London, Macmillan, 1894. Pp. XIV + 316.

Transactions of the Illinois Society for Child Study. Vol. I, No. 1. Chicago and New York, Werner Co., 1895. Pp. 73 + XLIII. 50 cts.

Imagination in Dreams and their Study. F. GREENWOOD. London, John Lane; New York, Macmillan, 1894. Pp. IX + 198.

The Study of Ethics. A Syllabus. J. DEWEY. Ann Arbor, Register Publishing Co., 1894. Pp. 151.

Elements of Psychology. Syllabus of Philosophy, I. J. H. HYSLOP. New York, Columbia College, 1895. Pp. 131. $1.

Popular Scientific Lectures. E. MACK. Translated by T. J. McCormack. Chicago, Open Court Co., 1895. Pp. 313. $1.

The Psychology of Childhood. F. TRACY. Second ed. Boston, Heath, 1894. Pp. XIII + 170.

Twelfth Annual Report of the Bureau of Ethnology (1890–'91.) J. W. POWELL. Washington, Gov. Printing Office, 1894. Pp. XLVIII + 742.

Logic. C. SIGWART. Trans. from second German edition by H. Dendy; 2 vols. London, Sonnenshein; New York, Macmillan, 1895. Pp. XII + 391 and VIII + 584. $5.50.

Lehrbuch der Psychologie. W. VOLKMANN. Edited by C. S. Cornelius. Fourth ed., Bd. II. Cöthen, Schulze, 1895. Pp. V + 568.

PSYCHOLOGICAL QUESTIONING.

ON OUR EARLIEST RECOLLECTIONS OF CHILDHOOD.

We possess very few observations on our earliest recollections. I should like to make a series of observations in this subject. I shall be grateful to all persons who will send answers to any or all of the following questions:

1. Age and usual occupation.

2. Do you have good *visual* representations of object in general; viz., can you form a visual image of an apple or of a lamp, etc.?

3. Do you have good *auditory* representations (of sounds), viz., have you auditory representations of the voices of your friends?

4. What is the earliest recollection of your childhood? Please describe it as fully as possible. How clear is it, and what was your age when the fact recollected occurred?

5. Had this fact a particular importance in your life, and if so, in what way?

6. Has anyone ever related this fact to you, or do you remember it yourself?

7. Can you give any explanation of this recollection, and if so, what?

8. What is the second recollection of your childhood? How far apart are these two in time?

9. Of what period of your life do you first have many recollections without connecting them in the time series of your life? How do they appear; are they clear, are they visual or auditory, etc.?

10. From what period of your life do you begin to have recollections of the time series of your life?

11. Do you ever have recollections of your childhood in your dreams? If so, what?

Please send the answers to these questions to Victor Henri, Leipzig (Germany), Johannis Allée 12. II.

NOTES.

Mr. J. S. MacKensie, M.A., has been called to the chair in Philosophy in University College, Cardiff.

The *Année psychologique*, of which announcement was made in an earlier number of this REVIEW, will be issued in March, 1895. The subscription price (7 fr., instead of 5 fr., as previously announced) may be sent directly to M. Alf. Binet, 29 Rue Madame, Paris, France.

We have received Bd. I, Heft 1, of a new serial publication, edited by Prof. E. Kräpelin, of the University of Heidelberg, entitled *Psychologische Arbeiten* (Leipzig, Engelmann, 5 M.)

The attention of readers of the REVIEW is called to the special announcements made by the editors on the second cover-page of this number.

Vol. II. No. 3. May, 1895.

The Psychological Review.

PRELIMINARY REPORT ON IMITATION.[1]

BY PROFESSOR JOSIAH ROYCE,

Harvard University.

In calling these few notes a Preliminary Report, I have deliberately wished to ask for all the indulgence which the phrase itself can properly invite. I have none but tentative considerations to present. I mean to tell something of the mere plan and programme of a research which I have ventured to begin, but which I cannot hope ever rightly to finish, concerning the processes that enter into the structure and growth of our imitative functions. In making my statement, I shall first be led to speak, perhaps at far too great a length, about the difficult problem of the possible classification and definition of the processes which we can call, with more or less right, imitative. I shall do so because my experimental work, later briefly sketched, has already suggested to me, not only the need, but at least one motive of such a classification. Then I shall very briefly indicate the first beginning of an experimental study of some simple imitative processes which I have been prosecuting only since October 1, at Harvard, under the guidance of my colleague, Prof. Münsterberg. If my little sheaves are, so far, very naturally lean, they may still suggest the fact that in this field the harvest is plenteous, whatever you may think of any of the laborers.

[1] Read at the December meeting of the American Psychological Association, at Princeton.

I.

If we ask ourselves: 'what is the definition of Imitation?' we soon find that any effort to separate imitative motor functions, in ourselves, from those which are not imitative, or imitative conscious processes from those mental processes which are not imitative, is a very difficult thing. Aristotle, who first undertook to define the psychological category of imitative mental processes, and of their correlative expressive functions, was quickly led to extend the term imitation until it came to include the most original productions of that poetic art which he himself called more philosophical than history. No student of the subject can easily avoid a wide extension of the category in question. Prof. Baldwin, in defining imitations (from a general biological point of view), by the very wide characterization which identifies them with 'circular' reactions, or with 'motor processes that tend to reproduce their own stimuli,' has seemed, I suppose, to many of his readers, to have made the concept of imitation far too inclusive for psychological convenience. All acts of sensuous attention are, of course, such circular motor processes. Again, is the insistent brooding of a mourner, as such, an imitative process? Yet it, of course, involves usually many circular reactions. Yet, from another side, there are facts that Prof. Baldwin himself has noted in passing, and will regard as familiar, but that would seem to tend to make even a still wider definition than this one desirable. In the laboratory experiments, later to be mentioned, I ask my subjects to listen to a rhythmic series of taps (made with an electric hammer), and then to reproduce this series by means of an electric key. Now, suppose that a subject persistently guides himself, in this process, as one of my subjects has often done, and as, no doubt, many people would do, by first, more or less deliberately, translating the series of taps to which he listens into a visualized image of pencil strokes arranged at intervals on paper, or of points marked off on a line; and suppose that he hereupon regularly makes his key-imitation by translating back from the visualized space intervals, and perhaps from the muscular feelings

of making the visualized pencil strokes into those muscular movements which reproduce the sound at the key. I suppose we should all agree that here not only is the final reproduction of the original series an imitative process, but the translation into visual and muscular terms, which serves as an intermediate instrument, is itself already an imitation. But, if so, the intermediate stage, which, be it noted, was not, in the subject noted, a spontaneous accidental association, but which was the gradual and habitual outcome of all the motor processes of his careful attention, and which arose as an incident of his deliberate effort to reproduce what he heard,—this intermediate stage is surely not itself the result of a function that reproduces its own stimulus, but of a function that produces, in image form, contents which are not those of the stimulus, but which have relations similar to those presented in the regular stimulus. But that imitations are thus often translations, reproductions which do not even mean to bring back the original stimulation, but which do mean to interpret it, by setting over against it its illumining counterpart in terms of some other set of stimulations, this, at least, on the highest levels of consciousness, is a commonplace. Aristotle already told us of art as intending to imitate life by producing before our eyes something that is in pretty marked contrast to life, e. g., an heroic tragedy. My subjects, at the key, as you will later see, find themselves both voluntarily and involuntarily doing a good deal of this same general sort of thing,—their imitations being often essentially interpretations, just as a translator imitates the original text precisely by meaning to write out, not the words which he gets as a stimulus, but the words of another tongue, which may, as faithfully as possible, serve the same ideal purposes. But in the same way, apart from the special motives of the translator, we all have very deep-set habits of imitating the sensations of one sense by means of deliberately recalled images belonging to another sense, and so, by motor reactions that tend to reproduce the stimuli to which these latter images correspond.

Thus the effort to define imitation, whether by wide or by narrow phrases, is at every point met with pretty decided

difficulties. Ignoring, as far as possible, any but the psychological point of view, I may, however, venture at this stage to suggest some of the more prominent classes of imitative functions, and of functions more or less obviously related to imitation, classes such as I myself have been led to distinguish. Whether any convenient generalization, as to the extent of the word imitation, is possible, we may then briefly consider.

To us all the word imitation first suggests motor functions, such as those of the child that struts about as a soldier, or that runs on all fours as a dog, or that learns to talk. Such functions are very numerous. We observe them in many animals, including birds. Their characteristic is that the imitator is more or less clearly aware of a model, and finds his own body more or less able to repeat certain usually extensive and complex movements of this model. This repetition gives satisfaction to the imitator. Imitation of this sort is to be roughly classified as either more or less critical. Sometimes the imitator is content with the roughest reproduction. Sometimes he is cautious, and is watchfully anxious to do precisely as his model does. A mocking-bird, as I at one period often observed in case of a household pet, appears to study with very great care at least some of the series of notes that he reproduces. Some children far surpass others in an early pedantry about the enunciation and use of their words. In any case, meanwhile, the subjective experiences of the imitator are here, at best, only in part identical with those given him in the stimulus presented to his senses by his model. He hears sounds, and replies by sounds, but of course he feels, more or less, the muscular and other organic disturbances incident to the reproduction. He sees the movements of his model. He both sees and feels his own imitative movements, and in all this he feels the latter as his own. In consequence, the imitator usually takes what is often called a decidedly 'subjective' sort of interest in his power to imitate. His activity has thus two strongly contrasted aspects. He watches his model, so far as he watches it at all, with a highly objective faithfulness. So far, his imitation depends upon a theoretical and very self-

surrendering sort of outward scrutiny. On the other hand, he delights in his own imitative powers as his own, i. e., as corporeally interesting events in his own organism, just as even the mocking-bird very obviously does. On this side the activity is, in the popular sense of the word, as self-centred as is eating or catching prey. That is, it is an activity whose conscious aspect involves an interest in inter-organic experiences. And, on this side too, the imitative process, in our children, is a great meeting place about which all sorts of self-considerate and self-conscious interests gather. Thus one sees how highly inter-organic, or subjective, as well as how highly outward-looking, objective, the imitative consciousness in the present class of cases has to be. Hence the enormous fecundity and various outcome of such imitative interest. Vanity and conscience, ideal devotion and flippant mockery, tame subserviency and the loftiest originality,—all these tendencies alike may, and in fact normally do, take root in this fruitful soil, and any of them may grow into the child's later character, and all because he was, in the first place, disposed to repeat the complex motor processes of his models, and so was forced to set off his consciousness of his own movements against his perception of the movements of others, thus emphasizing both his ideas of himself and his ideas of his models, each set of ideas by contrast with the other set. Imitation may thus become, to use the words again in their purely popular sense, the most self-abnegating or the most self-considerate of tendencies, according as, in the end, one or the other of these opposing drifts of attention gets emphasized, i. e., according as one comes to consider rather his own imitative organism or the outside model.

So much for a first and most familiar class of processes defined as imitative. But we all of us extend the word imitation to include those intelligent functions which tend to the voluntary production of external objects resembling certain other objects called the models of the objects produced. Thus, drawing, painting, modeling, building, mechanical skill of all sorts, are universally named imitative functions. In our own cases such functions, as a class, are obviously almost altogether derived, directly or indirectly, from the

functions of the former class just characterized. We learn, namely, to reproduce things, in whole or in part, by first having learned to imitate people. Mechanical skill may early become self-directing. But it is probably always, at the start, socially guided. Psychological complications are, accordingly, here of much the sort as in the foregoing class of cases. Our imitations of objects involve vast numbers of relatively controllable conscious processes besides our perception of the finished products which resemble the stimulating models. And here, too, in consequence, both our relatively objective, or outward-looking, and our relatively subjective, or inward-looking, interests get a correlative development as we learn to imitate—a development which may have the utmost complexity, and the most momentous psychological consequences. On the whole, however, the imitation of things generally tends, as they say, to send us 'out of ourselves,' i. e., outside of our interest in the processes of our own organisms, still more than does the imitation of the mere acts of people. Our imitative deed is transient; but, when we make something by the deed, its product here remains to calm our more anxious or our vainer interest in our own motor processes as such. Hence it is that musicians are more subjective in mood than are architects; and it is easier to be vain about matters of social etiquette than about one's skill as a carpenter, in case one has any such skill; while, to pass to another case where imitation is complicated with originality, nobody can judge his own book while it is in press as he can after it is in cold print and binding before him.

Now, I have laid stress upon the factors present in these two classes of cases of imitation, because I have meant to use them to illustrate the general nature of imitation itself. In these two classes of cases imitation is not merely, as a psychological process, the reproduction of a series of sense stimulations, or of external perceptions by means of a series of motor processes; but it is something still more complex. It is not only a process by which we reproduce one set of data by means of another set of data like the first, but it is also a process by which we get two sets of data whose inevitable

contrasts are as interesting and as instructive to us as their purposed resemblances. We get an interpretation of the perceived model through the imitation of it. On the other hand, to say that imitation, in these cases, is an act whose main motive is to interpret my perceptions by means of my deeds, is indeed true; but of course, so far, the same might be said of all those acts, such as looking, listening, approaching an object, grasping, touching, handling, exploring, in the perceptive field,—of all acts, in short, which involve intellectually valuable motor processes. What, then, is the characteristic feature of the imitative acts in the mentioned classes of cases? Does it not obviously lie in the fact that my interpretation of what I am usually said to perceive outside of my organism, in the external world, is, in the case of these classes of imitations, conditioned upon my setting over against my perceptions a series of motor processes, or of perceived results of motor processes, which in its wholeness contrasts with the other series in the one principal fact that the motor processes, the imitative deeds or their results, appear to me relatively controllable, plastic, reproducible at will, while otherwise the two series are largely similar. When I learn to grasp an apple, the grasping is indeed, once learned, an easily reproducible and so controllable deed, and on suggestion is remembered as such. But when I learn to say *apple*, upon hearing the word pronounced, the act, once in my power, is felt as controllable, but as to result it resembles its model (namely, the word apple, pronounced by my neighbor)—something that concerns not its controllableness, but some of its other characters. Thus, in these cases, imitation is definable, from the psychological side, as an act that interprets an uncontrollable perceptive series by setting over against it a series of experiences that appear to be similar to it in content, but to be also in contrast with it by virtue of their controllableness. Or, again, an imitation is an act that tends to the interpretation of what is beyond my power, or is independent of my movements, by contrasting it with what otherwise resembles it, but is in my power, and is a result of my movements. This feature of imitation, viz., that it accomplishes the aim of throwing light on the

uncontrollable percept by setting the controllable deed beside it, is, I suppose, the principal intellectual function of the higher imitative life. That the light thrown on the processes is throughout relative, that what I perceive outside me helps me to know, by contrast, my own imitative act, as well as the latter helps me to know the former,—this, after what has been said, needs no further illustration. At the outset, of course, we make no clear sundering between what goes on inside our organisms and what we perceive outside them. My point here is, it is our imitation that helps us first to do so by first bringing the mentioned contrast to light.

Now, however, as helping us on to another class of imitative functions, we may note the fact that where I thus use imitative processes to set off or to interpret perceived facts that are outside of my organism, it is not necessary that the similarity between the externally observed and the internally produced processes, between the original and its so-called copy, should have any one established or even desired degree of closeness. I insist, it is often the contrast as much as the agreement between the two that interests us. In every case so far the imitation differs from what it copies by virtue of the associated muscular and affective accompaniments which make the imitation our own, as distinct from what we merely observe without. These accompaniments may involve all the emotions of play. In that case the imitator very frequently wants his imitation to be unlike as well as like its original. One plays in one's own original fashion. Mocking is often more or less consciously untrue to its model. 'This is what you do,' we say to the person whom we mock. But thereupon what we do is only a pretended imitation—an exaggeration, of whose grotesque unreality we then make an ideal. Children surely often do this. The reasons for such action lie deep in the nature of the play motives. The mocking imitation is as imperfect a copy as are often the actions of kittens at play, when compared with the behavior of grown cats that are seriously fighting.

If an imitation thus often sets off our consciousness of the original by virtue of the very contrast that mingles with the similarity, it is plain that we may look to find imitations that

not only by accident, but intentionally, represent one set of sense data in terms of activities that give us data belonging to another sense or to any otherwise contrasted group of experiences. The imitation of a series of sounds by a series of movements involves, of course, as in dancing or in beating time, a vast number of acquired habits of conplex nature. Yet the fact remains that such imitations do both fascinate and enlighten us. This principle of the tendency to deliberate idealization of our imitations, to deliberate deviations from the literal, one finds, then, in the most varied forms, in play, in art, in the far-reaching and deep-seated tendency, very complex in its origin, to translate space-relations into time relations and *vice versa ;* in every form of fondness for what one may call symbolical motor processes, and so, finally, with very momentous consequences, in all those motor processes that are connected with the growth of our theoretical thinking. That our thoughts are, in this general sense, conscious processes by which we constantly mean to imitate the truth of the things that we experience, is perfectly obvious. Equally obvious is the fact that to think experience is to translate it into terms which are decidedly foreign to its character as it comes to us, apart from such ideal reconstruction, and in its first intention. Now thinking accompanies motor processes, abbreviated and truncated and rendered abstract in all sorts of ways, but very obviously and highly imitative in all the cases where we get them in any relatively unabridged form. The gesture language is a case in point. It gives the gesturer trains of experience of a very complex character, which are in a summary and more or less symbolic fashion similar to the primary trains of experience which by his gestures he undertakes to describe. For gestures we who speak have now learned to substitute trains of words, which we follow with an endless chain of attentive processes shifting from one series of images to another. But the series of attentive processes, as it follows now these, now those images, gives us a total inner experience which we call an account of the experienced reality beyond the thinking process. The value of this account we judge by its resemblance, not in detail, but in its total net-

work of related elements, to those aspects of the relatively external experience which our thinking means to emphasize. And yet, on the other hand, how unlike their originals our abstract ideas mean to be. How far is the thinker's imitation from being a mere inner reproduction of the external experience about which he thinks? It is the very contrast which here enlightens us, when it is accompanied by a consciousness of the sort of agreement which we all the time intend. In symbolic imitation the imitative subject means to neglect all of his model except his own chosen aspect of it; and even this aspect he generally means to reproduce in terms of a sort of inner experience which differs from it as widely as the data of one sense can differ from those of another.

So far I have mentioned ordinary imitations of the doings of our comrades, acquired tendencies to reproduce or picture things, and then the endlessly numerous cases of consciously idealized, playfully falsified or symbolically abbreviated imitations of the interesting aspects of things. Is it not fair to call all these manifestations of the imitative tendency? But some one will say, as people have said of both Tarde's and Prof. Baldwin's uses of the term imitation, that to go on in this fashion is in the end to include pretty much all psychical processes in the field covered by the word. If imitation occurs wherever there are relatively inner or organic experiences—*e*, *g*., images or trains of images which, in some aspect, resemble certain relatively external or perceptive experiences—then where can one name an experience involving any images whatever, or any organic adjustment, which will not have something imitative about it? I reply that, if the foregoing classes of cases were all that I had to consider, I myself should be disposed to draw the lines about the class of processes to be called imitative from a purely psychological point of view, in this way: An imitation either is or accompanies a sort of motor adjustment. And, now, what sort? I answer: So far as we have yet gone, an imitation appears as an adjustment that leads to the emphasizing or interpreting of a train of relatively external experiences, by virtue of the fact that the mental accompaniment

of this adjustment is a train of relatively inner experiences (muscular feelings, or images of any sense you please, or affective states), while the similarity of the train of internal experiences to the train of external experiences serves, in the midst of the mutual contrasts of the two trains, to make livelier the consciousness of each series, when viewed side by side with the other. Or, more briefly, an imitation is a more or less conscious motor adjustment that tends to set off a series of given experiences by furnishing from within the conscious counterpart of some one or more of the aspects of the first series—a counterpart which is both like and unlike the original, and whose contrast is therefore often as instructive as its similarity.

Essential to this notion of imitation is so far the fact that the consciousness of the imitator is as truly a consciousness of his adjustment as it is a consciousness of his model. To be sure, at the outset, an infant has no clear idea of himself; but the point is that the ideas of inner and outer thus get clarified. The two must be more or less clearly held apart. How clearly depends upon what grade of consciousness you are considering. Moreover, the model is not a simple sense-fact, like a color, but is always, where we speak of imitation, a complex series of facts. We imitate the complex. We may by mere association reduplicate the elementary, but in that case we have no true instance of imitation. Where association by similarity takes place between a relatively elementary fact of sense and an image, there is no imitation: (1) because one isn't at all conscious of this association as involving what we call his motor adjustment as such; (2) because in many cases the associated elements tend to blend, and not to set each other off; and (3) because by imitation we always mean a consciously complex process of adjustment. Imitation, in the classes of cases heretofore considered, does not mean, therefore, mere similarity of relatively inner and relatively outer experiences, but the similarity of a complex motor series or of its complex result to a complex perceptive series, the conscious interest lying in the antithesis of the two as well as in their mutual support. The two are not merely alike, but each is more or less consciously

referred to the other. The imitation *means* the model, as well as chances to resemble it.

But, if we now proceed one step further, we do indeed seem to meet with functions which an external observer calls imitative, but which apparently do not conform to the foregoing definition. Many of our imitations occur with very little consciousness. We sit when others sit, rise when they rise, yawn when they yawn; follow fashions without any clear intention to do so, and catch by contagion tricks of gesture and facial expressions, as well as states of emotion. Panic-fear, in all gregarious animals, involves functions that seem clearly imitative. Yet here one surely does not mean to observe either the likeness or the contrast between the outer and the inner experiences. In fact, a contagious emotion, such as terror or a violent sympathetic faintness at the sight of another's pain, often seems rather to forbid the appearance of any clear or conscious sympathy with one's fellow as an objectively real person, and one gets lost in one's own feelings even while one is imitative. Yet even here, although the antithesis leaves consciousness, and the relatively subjective series of inter-organic processes and experiences does not help one to interpret the relatively external facts in any deliberate way, it still remains true that we have the one series emphasized by and dependent upon the other; and while the imitator himself does indeed lose sight of any clear relation of himself to his model, the external observer calls this an imitation, and not, like the independent nest-building of two birds of the same species, a mere resemblance in function, because the observer can see what the imitator neglects—the close relation of dependence between the two resembling and contrasting series.

Now I, of course, cannot doubt that, biologically speaking, the tendencies towards a relatively unconscious conformity of an animal's conduct to the conduct of its herdfellows, lie deeper than the more conscious and intelligent sorts of explicitly discriminating imitation which I have so far defined in this paper. But the question still remains as to what it is about these relatively unconscious sorts of imitation which makes them the basis of so much that is later

important for the higher psychological functions. I venture then still to point out that the unconsciously imitative gregarious animal is still going through motor processes, such as place, side by side with various series of his sensory stimuli, a great number of inter-organic series of processes. These processes, on the one hand, extend far beyond the mere adjustment of his sense organs to the stimulation, while, on the other hand, they tend to emphasize these sense stimulations, not merely by repeating them, but by giving them companions which in various ways resemble them, and which therefore make them more effective in leading to further conduct. When chickens suffer from contagious fright, they all repeat the warning cry of the flock. Of what use is the repetition? Each fowl is in consequence warned, not only by his neighbor but by himself, and the inner warning comes not only to the ear, but also through just those motor sensations and affective inner states which accompany this motor adjustment. No other fowl could warn this one as the bird can warn itself. What is heard without already puts each fowl somewhat on the alert. But the inner resonance of the imitative act makes far more impressive this whole experience of danger. So then, even here, an imitative act appears to me to be not so much an act that, in Prof. Baldwin's phrase, tends to repeat its own stimulus, as an act that tends to reinforce, emphasize, signalize, clarify its complex stimulus by adding thereto other and parallel series of internal or organic stimuli, which by their similarity as a series shall support, while by their differences they shall in general supplement, the stimulus in question. This inter-organic imitation and supplementing of one series of stimuli by a series of inner experiences is, as a fact, very naturally connected in similarly organized beings with a behavior that, externally viewed, appears imitative, even when the creature in question is not interested in this imitative character. Simple attention need not, from my present point of view, be regarded as involving imitation, although simple attention does involve a circular reaction which tends to the repetition of its own stimulation. But if attention is supported by the appearance of a series of experiences,

motor or emotional, which are produced through the motor adjustment, and which taken together, run parallel to a series of stimuli and resemble it, thereby emphasizing and supplementing it, then one has an imitative function.

I conclude, then, so far in general, that imitative functions seem to me to be those which tend to emphasize, to support, or on higher levels to interpret, a complex series of sensory stimulations, by producing, as their accompaniment, another series of inter-organic experiences which resembles as a whole the first series, but which involves in general decidedly different activities of the organism, in addition to those of the organs receiving the stimuli. Lower cases of imitative functions already show us the contrast, in the midst of the similarity, between the imitative process and its sensory stimuli. Higher up this contrast is itself made an object of consciousness. Imitation and model are contrasted series of presentations whose relation keeps them apart. And hence it is that, as I myself suppose, imitation is, psychologically speaking, the one source of our whole series of conscious distinctions between subject and object, thought and truth, deed and ideal, impulse and conscience, inner world and external world—in short, of all those familiar and fundamental rational distinctions which psychology has hitherto found so baffling. The contrast between model and imitation is, to my mind, the first appearance in consciousness of that differentiation which in the end makes internal and external experience not merely qualitatively different—as, of course, they more or less are from the first—but consciously discriminated, as at first they seem not to be.

Biologically speaking, I should fancy that imitation might be, in the end, explicable by something even more fundamental still than Prof. Baldwin's circular reactions, viz., by those generally coöperative tendencies which must lie at the basis of all evolution in organisms consisting of multitudes of cells. These involve amongst their number tendencies to direct functional agreement. What occurs in one part of an organism must in general be repeated with variations elsewhere, in so far as the cells of various regions may be of the same general type, and are meant permanently to

coöperate. Such inter-organic repetitions of disturbance (attended with wide contrasts, which run side by side with the functional agreements) we have in all those recently much studied physiological accompaniments of emotion; and in all those phenomena of functional nervous equivalents which attract one's attention in the history of the varying symptoms of many a complex nervous case. Here what happens to one set of cells may tend sooner or later to be represented by a more or less contrasted functional equivalent in some other set of cells. Now these things are not yet cases of imitation. But they suggest a basis upon which imitative functions may have grown.

I said that my few experiments have already, without as yet proving anything, suggested to me the need of some such analysis as the foregoing. The scope of the experiments themselves is comparatively narrow. Yet some of you will perhaps think it already too wide.

I have desired to get some notion of the inner consciousness and of the outer effectiveness of a person engaged in acquiring skill in some imitative process. This process, as I desired it, should be fairly simple, and yet complex enough to involve the coöperation of a number of different habits, interests and forms of attention in the accomplishment of one end. I decided, by Prof. Münsterberg's advice, to choose the process of imitating rhythmic series of taps which were to be made at controllable intervals by means of an electric hammer, and imitated with an electric key by the subjects. In choosing the particular series of taps, I have followed my own choice and responsibility, and must confess that I have tried several rhythmic series that any more experienced psychological experimenter than myself might have easily regarded as too complex to promise any definite results. Yet so far, despite various inevitable eddies in my little stream of experience, I have not been disappointed at the wealth of suggestions that have come to me. I have regarded the so-called time-sense aspect of my experiments as a necessary, but for my purposes a very subordinate, aspect. The nature of the rhythmic consciousness itself comes in my way, but rhythmic consciousness is here only the instru-

ment, not the end. The chief aim for the first has been to get a pretty careful series of records of the facts, and to wait for experience to indicate the best further procedure. The facts collected have so far been objective records of the imitations and a constant series of subjective records written down at once, after such experiment, by the hands of the subjects concerned.

As for the physical mechanism used, a mechanism which, as I frankly confess, better hands than mine generally guide, it is in summary this: On the axle of a kymograph drum wheels revolve armed with platinum points, arranged at pleasure for each rhythmic series as used, the points successively dipping into mercury contacts at the lowest point in each revolution of their respective wheels. The completed contact gives in each case one stroke of an electric hammer. The moment of each stroke is recorded by an electric pen on the kymograph drum, the record itself being controlled by a tuning-fork tracing. Any one rhythmic series having been heard through by the subject (who sits holding, ready for his response, a metallic key especially prepared for these experiments), the subject, at the word 'ready,' repeats the rhythm that he has heard, by making successive connections between the point of the key and a mercury contact beneath. The key itself is arranged so as to be noiseless, or nearly so, in its own movements. At times it is arranged in the same circuit with the hammer, and then the subject, in making the contacts, tries to repeat the very sounds which he has heard and at the same intervals. At other times this connection is avoided, and then the subject makes his imitative contacts with a 'silent key' depending on the inner light only. Every imitative series of key-contacts is recorded on the same drum with the rhythmic series of hammer strokes which was to be imitated. The routine of each experiment is simply that the kymograph is started; the subject, who cannot see, although he does indeed hear the rotation of the mechanism, hears the word 'ready,' and then the series of hammer taps to be imitated. These taps, of course, cannot under these circumstances be made perfectly uniform, owing to the uncontrolla-

ble variations of the hammer's relation to the magnet; but they have no regular emphasis, and subjects learn to ignore the more ordinary of the caprices of the hammer. The rhythm being completed, there is a very brief pause for readjustment, when the subject, at the repeated word 'ready,' proceeds to beat off on the key as exact an imitation as he can of what he has heard. He then at once records dated and numbered notes of his subjective experiences during the experiments, and the records are filed.

So far most, although by no means all, of the records have been taken in work upon two rhythms, both complex enough to make the labor of apperceiving and reproducing them with relative exactitude decidedly noteworthy. They have first been learned, then practised upon daily, or as often as possible, their rates being very widely varied, while keeping the relations of the intervals constant. Separate series of experiments upon the estimation of slight changes of rate, apart from imitation, have also been recorded. And a considerable number of records have been taken of the skill of the subjects in independently giving and varying by minimal steps each rhythm after it had been learned. Of late one of the rhythms has been deliberately distorted by introducing irregularly placed new points into three of its more noteworthy intervals; and the vast change thus suddenly introduced into an already well-established series of conscious data has been studied, both objectively and subjectively.

The subjects include at present four women and four men, all of a fair although decidedly varied amount of introspective preparation. Three have a fair musical training. One of these is especially delicate in rhythmical perception. One of the unmusical subjects, on the other hand, is especially imperfect as to all clearer rhythmical consciousness. Another is a Japanese. Questions have been asked for the subjective records as the state of the experiments seemed to indicate. Above all, I have wanted to know what it means to the subject to try to catch, to hold, to make an ideal for action, of this series of monotonous taps with their varying intervals. I have now about 200 of these subjective notes,

corresponding to about 1000 repetitions of the various rhythms.

Well, so far, I have been especially struck by the fact that the process of holding for imitation involves, according to the records, the most widely varying subjective processes, which do not seem to be constant, even for one subject, in any such way as you would expect. One catches the rhythm and prepares to repeat it by means of what appear in consciousness as the most heterogeneous materials. There is first, of course, the case where one tries, voluntarily or half involuntarily, devices which are either consciously abstract sorts of imitation, such as counting, or else involve the use of voluntary muscular movements, of hand or of foot, made in time to the rhythm while one hears it. But curiously enough, in many cases, and with some of the subjects, devices of this kind are felt as rather hindering than helping the imitation. Interesting also, with some subjects, is the lack of any preference for any particular set of these voluntary or semi-voluntary motor devices. But next, side by side with these voluntary processes, or instead of them, there appear unconsciously selected masses of varying organic feelings, which seem to be quite involuntary in their special origin and which are at least nearly always unexpected. These, when they come, keep some sort of time with the rhythm, and may help to apperceive it. They are described as inner beatings, 'in the head,' 'in the neck,' 'in one temple,' 'in the ball of my thumb,' as tinglings, throbbings, or what not. These vary most remarkably from experiment to experiment, appear to vary quite apart from one's expectation, to come and go as they choose. To these are joined on occasion all sorts of involuntary associations of a more or less symbolic sort—'ideas of urgency,' or of 'deliberation,' or of merriment, or of other such sorts familiar to all who note musical associations. Visual associations join themselves—a dark rhythm has been mentioned in one case. The visualization of the intervals as space intervals is not unknown. All these phenomena show so far a rather baffling variety, which forbids one easily to reduce them to the terms of habit. The whole process, at least in all its

earlier stages, show far less routine than I had expected. The report of definable 'waves of attention,' as such, has been rarer than I should have anticipated. Perhaps further introspection will distinguish these facts better. But, of course, when the rhythm is once well learned, all the foregoing processes may and sometimes do lapse into a mere sense, that 'I know all this.' Then, however, one still has a model general idea or ideal of the one rhythm, 'just like a sentence,' as the subjects are wont to say—a general idea of the one rhythm, which is still variable as to its tempo. By as elaborate devices for variation of the facts as I can devise, I am just now trying to run down what this general ideal of the variable unity of the one rhythm really is. But to speak of this would take me beyond my space. Nor have I as yet any report to make as to the time facts of the rhythm experiments.

I have meant to state a problem, viz., that as to the essential nature of the processes called imitative, and to report the mere fact of a research now under way. You may see how one of these reports suggests why the other is an indication of matters worthy of further study. Herewith my present purpose and your time are alike exhausted.

STUDIES FROM THE PRINCETON LABORATORY (I–V.)[1]

I. Memory for Square Size.

BY J. MARK BALDWIN AND W. J. SHAW.

The experiments of this study were performed at Toronto by Prof. Baldwin and Mr. W. J. Shaw, during the winter of 1892–3.[2] The object was to determine the accuracy of the memory for *size*, as affected by the lapse of time. A figure of two dimensions was selected for experiment because of the tendency to measure linear size in terms of well-known units of length. Circles tend to be measured by their radii, but in the case of the square, the impression is that of the area, and the natural memory-image is not so liable to be corrected by comparison with standards fixed in mind by repeated experience.

The experiments proceeded by three different methods: (1) *Selection from a Variety.* A single figure (the *normal*, 150 mm square) was drawn on a black-board and shown to a large college class; after a certain time a number of squares of various sizes were shown simultaneously, and the class was requested to designate the one that appeared to be the same size as the normal. The squares ranged from 130 to 210 mm by intervals of 20 mm, and the time intervals were 10, 20 and 40 minutes. The class consisted of about 225 persons, of whom some 50 were ladies. (2) *Identification.* Here the normal square was first shown, and afterwards one other square; the subjects were asked to say whether the latter appeared to be greater, equal to, or less than the normal. The time intervals were the same as before, and

[1] These studies were all concluded in the college year '93–'94.

[2] Reported in these words to the Amer. Psych. Ass., Dec., 1893, by H. C. Warren.

the second square was in every instance 20 mm greater than the normal.

Both series were treated by the Method of Right and Wrong Cases, and the two results showed remarkable agreement. The percentage of right cases is shown in Table I.

TABLE I.

	I. By Selection.	II. By Identification.
10 min. . .	64.1	87.6
20 " . .	59.3	82.7
40 " . .	36.4	58.5

Plotting the results (Fig. 1), we find the *memory curves*, as they may be called, practically parallel, but the degree of accuracy is much higher by the second method than by the first. In each there is a rapid falling off at first, then a period of gradual descent, and finally another rapid drop.

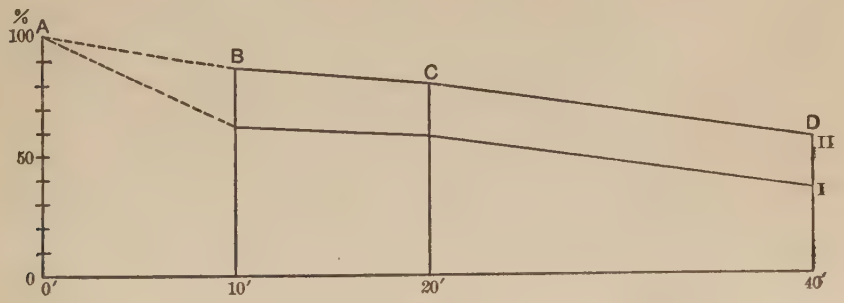


Fig. 1

The greater accuracy of the results in II is partly due to the manner of stating the question. Should the memory-image of the normal square either remain unaltered, or decrease in size, the subject would respond correctly that the second square was the greater, and he would respond incorrectly only if his memory-image had *increased* sensibly in size from its original. Whereas, in the series by Selection his responses would be classed as incorrect if his memory-

image had *either increased or decreased* sensibly. A further source of error in the series by Selection was the disturbance due to simultaneous contrast between the figures. Some special experiments were afterwards made to determine the effect of this contrast (see Study II, below).

In discussing the form of the two memory-curves so reached, it must first be observed that their real origin is not at A, but at a point, or points, near B. For the difference of 20 mm is very much greater than the least discernible difference between two squares observed in immediate succession; hence, even if a considerable interval should elapse before the second square is shown, *no incorrect judgment will be given.* The effect of this is to make the first falling off, when once it begins, even more rapid than indicated on the diagram, and possibly also to carry out the parallelism between the two curves still further. The reason for the sudden falling off may lie in the conditions of the experiments. The subjects began to take notes on a lecture immediately after the normal square was shown, and there was consequently a sudden withdrawal of attention from the memory-image, allowing it to fall into great indistinctness at once. After this first influence had taken effect, there was, it seems, but little change until the ordinary factors which tend to make the image more vague, began to take effect. The work of these factors, which one would scarcely expect to become apparent within 40 minutes, may have been hastened by the fatigue arising from steady application.[1]

(3) The third series proceeded by what was termed the *Method of Reproduction.* A normal square having been shown, as before, the subjects were asked, after the stated interval, to draw a square of the same size on paper. The normal in this case was 170 mm square. The reproductions were almost always too small, their average being 146.0 after 20 min. and 146.4 after 40 min. This result was rather unexpected, as the other series had indicated a tendency of the memory-image to increase in size beyond the original. It may be attributed to two factors: (1) The muscles of the

[1] The results were examined for a possible difference between the two sexes, but the variations were neither marked nor constant in direction.

hand were fatigued from continuous writing, and this tended to give the impression of a figure larger than that actually drawn. (2) The paper on which the drawing was made was not much larger than the actual size of the normal; any figure coming close to the edges would appear very large, since it occupied so large a portion of the field. Hence there was a tendency to draw the square too small. On this account it was decided to separate the results obtained by this method from the others, where the conditions were more nearly alike.

II. Further Experiments on Memory for Square Size.

BY H. C. WARREN AND W. J. SHAW.

The experiments were taken up at this point by Messrs. Warren and Shaw, at Princeton. A possible objection to the Selection Method lay, as has been said, in the disturbing influence of simultaneous contrast. To investigate this, the following experiment was performed: Ten squares, ranging between 100 and 190 mm, were drawn in promiscuous order on a large sheet of paper; on another sheet of the same size a single square was drawn as normal, and the two sheets were placed in different rooms. The subjects observed the normal first, and going at once to the other room designated the square which appeared equal to it. The normal used was 120 mm in one instance and 170 mm in another. In each case there was a marked *attraction towards the center of the series*, the average for the normal of 120 mm being 123.3, and for that of 170 mm, 165.

On this account it seemed desirable to supplement the Toronto experiments by others, and to employ a somewhat different method, using a series which combined the advantages of Selection and Identification. The object was to determine the threshold, *i. e.*, the (average) least perceptible difference from the normal after a given period of time. In each experiment the normal was first shown, and after the interval another square as near the threshold as the latter could be determined from the previous experiments; the experiments were continued until the threshold was found.

When the squares were shown in immediate succession (interval of *no* minutes = perception), the threshold was found to be 3 mm for squares of about 150 mm. When the interval was increased it was found to make an essential difference whether the second square was the larger or the smaller. For an interval of 10 minutes the threshold was 8 mm if the second was smaller, while it was but 5 mm if the second was larger; for 20 minutes it was somewhat less than 8 mm if the second was smaller, and less than zero (a minus quantity!) if the second was larger; that is, when two squares of the same size were shown, 20 minutes apart, *the second was pronounced the smaller* by over 50 per cent. of the subjects (actually, 63 per cent.)

That this result was not accidental (the conditions rendered any collusion impossible) was proved by the substantial agreement of all the experiments, pointing as they did without exception in the same direction. The entire series (marked *a* in Table II) was performed on the same subjects, a college class of about 50, Juniors and Seniors, on nine separate occasions, the 10-minute intervals being taken first. Besides this the table shows two experiments (marked *b*) on

TABLE II.

Interval and order.	Difference between I (normal) and II.						
	20 mm	12 mm	10 mm	8 mm	5 mm	3 mm	0 mm
ó . . . . . II < or > I	—	—	95 (c)	87 (c)	63 (c)	4 mm = 59 2 mm = 44 (c)	85 = (c)
1ó . . . . . II < I	—	—	—	—	50 (a)	—	—
1ó . . . . . II > I	87 (d)	—	70 (a)	53 (a)	—	—	—
2ó . . . . . II < I	—	—	75 (b)	94 (a)	75 (a)	65 (a)	63 < 24 = 13 > (a)
2ó . . . . . II > I	82 (d)	82 (a)	37 (b)	67 (a)	—	—	

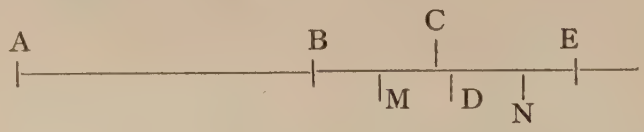
The figures denote percentage of right answers, except under 0 mm, where they denote the judgment (=, >, or <,) actually made. The normal was 150 mm square.

two other college classes of 50 and 65 respectively, where squares of 150 and 160 mm were used, with a 20-minute interval, the normal being smaller in the former case and larger in the latter. The lack of practice makes the threshold much greater in these instances than in the others, but they exhibit the same difference, depending on the order of sequence. The line of values marked *c* shows the experiments on squares immediately succeeding one another (0 minutes interval), taken with still another set of subjects, and the two values marked *d* are taken from the earlier experiments by Identification.

These results unite to show that besides the growth of inaccuracy, or indistinctness, in the memory-image, there is another factor at work, by which *the memory-image tends to grow larger* as the time interval increases. The table gives three cases which allow direct comparison between an increasing and a decreasing sequence: (1) With unpracticed observers (see *b*), 10 mm *increase* from the normal was noted by only 37% after 20 minutes, while the same amount of *decrease* was noted by 75%. (2) With practiced observers (*a*) ,8 mm increase was noted by 67%, and the same decrease by 49%. (3) With the same observers as (2), the final test, after considerable practice, was with two equal squares, separated by 20 minutes interval; 63% pronounced the second square smaller, 24% equal, and 13% larger. Comparing this with the observations on the threshold for perception, we see that while half of the subjects can distinguish a difference in the latter case only when it amounts to 3 mm, in case of a 20-minute interval a majority actually think they perceive a difference *when none exists*, indicating plainly that their memory-image has grown by more than 3 mm, apart from any increase in the extent of the territory lying 'below the threshold.'

These results are not so satisfactory as the earlier series (see Table I) for determining the actual law of the threshold, on account of the increased degree of practice as the experiments proceeded. But they bring out clearly this fact of the growth, or *exaggeration*, of the memory-image. The following explanation, based on direct deductions from Weber's

Law, is suggested to account for this exaggeration. As given here, it assumes Weber's Law to hold rigidly; but even if we accept the latter only in the modified form that *the increments of sensation grow less rapidly than the increments of stimulus*, it will be seen to apply as a constant tendency, and will produce the result indicated, if the supposition on which it is based be admitted.



Let AC be the normal stimulus, and AB, AE the stimuli just perceptibly different from it. Then $\frac{AC}{AB} = \frac{AE}{AC} = r$, a constant for the entire series, according to Weber's Law; and $CE > BC$. Now the central stimulation of the memory-image may assume any magnitude $>AB$ and $<AE$, and any image within these bounds may be identified with the memory-image in respect to size. As there is no objective regulation of the stimulus, the actual representations will tend to distribute themselves, according to the theory of chances, evenly between B and E; but the images around B and E, and decreasingly towards C, will tend to be rejected as too small and too large, respectively. As the memory-image becomes indistinct, however, the imagination-images nearer C are less frequently rejected, and at length no images will be rejected between two given points, M and N. Now since the actual reproductions are distributed evenly between M and N, and none are rejected, the *average* of these will tend to assume the function of memory-image; that is, the point D, midway between M and N, will tend to become the basis of judgment, since AD is the average of the unchallenged images. But, since CN is always greater than MC, the point D will always lie beyond C; *i. e.*, $AD > AC$. Thus there will always be a tendency on the part of the memory-image to grow larger, as soon as there is any tendency on its part to become vague or indistinct; and the continuation of this process will be limited only by the limits of the vagueness of the memory-image, or by its relations to other objects or

memory-images which afford a means of comparison and regulation. In the instance at hand there are no such means of regulation within very wide limits, and the exaggerating process goes on without hindrance, so that in the end the point B is transferred to a position beyond C,—a result which, while unexpected and remarkable, is fully accounted for by the above theory.

The close of the college year prevented an extension of these experiments to intervals of 40 minutes with the same set of men.

A word or two may be in place here regarding the relation between single experiments on a number of subjects and a series of experiments on a single individual. In any experiments where a number of results are combined and their averages taken, what is sought is a *representative* value. By multiplying the trials, accidental influences are eliminated and we obtain a value representative of the given individual under the given conditions. If the individual represents some peculiar type, we should further compare his results with those obtained from individuals of other types. If, however, what we desire is the observation of an *average individual*, we must make sure that our subject is such, by comparing him with others. Rather than repeat the entire series on several individuals, we may save time and labor by performing a single experiment on a number together. There are then a number of precautions to be taken. (1) Each subject must understand perfectly the nature of the judgment to be made. (2) The judgments must be entirely independent. (3) The subjects must be representative—not drawn from some one peculiar class; and they must be governed by sensibly the same conditions. (4) Finally, care must be taken with the *objective* conditions of the experiment, so that no vitiating circumstances shall creep in.—In the present instance, every precaution was taken to fulfil the first two and the last of these requirements, and, a number of doubtful results having been rejected, the remainder fulfilled the conditions exactly, as far as a most careful scrutiny and attention on the part of the two observers could determine. Further, the subjects were acted upon by sen-

sibly the same conditions during the given interval. There is, of course, room for variety of opinion as to how far representative a college class is to be considered, and what allowances, if any, should be made for differences in previous occupation and differences in location with reference to the platform where the squares were shown. The writers are inclined to minimize these differences, and as to the former question, it is urged that a body of men like those under consideration are perfectly representative of the average educated male of about 21. We believe the results to be far more satisfactory than a quantity of experiments on merely one or two individuals, and think that this *cumulative* method, under which alone are possible certain experiments involving a great amount of time, may safely be used in connection with the more usual procedure.

III. The Effect of Size-contrast Upon Judgments of Position in the Retinal Field.

BY J. MARK BALDWIN.

I. *Problem, Apparatus, and Methods.*—The indication given in a preceding Study (I) that the arrangement of squares of various sizes in the visual field has an influence upon the identification of one of them as of a certain remembered size, suggested a farther research. It occurred to the writer that any influence of contiguous squares upon each other would be accurately measured by their joint influence upon the subject's estimate of some other distance on the retina. And such a distance as that lying between the squares lends itself directly to this purpose.

An arrangement was readily effected, whereby the ratio of the sides of two squares to each other was varied in a series of values, while the distance between the squares was kept constant. Any regular variations then in the judgment of this latter distance, such as that of its mid-point,—*i. e.*, the bisection of the distance between the squares,—would be due to the variations in the ratio of the square-sizes. Such a problem shows practical bearings also in all matters which

require estimates of balance, division, proportion in right lines between masses, objects, etc., in the field of vision: such matters as the hanging of pictures, all designing of cuts, vignettes, architectural plans, etc., which involve line values. Of course all variations from the correct location of a mid-point, or other critical point, lying between two masses of material, color, etc., should be allowed for in applying the formulæ of æsthetic effect.

A further complication also arises when movement enters into the case: the movement of the contrasted masses toward or from each other, of the eye from one to the other along the line of connection, or of the element of this line whose evolution describes the line.

Experimental Arrangements.—The following description (with Fig. 1) of my device for investigating the problem is given in some detail, since it meets the essential requirements of such experimentation and is so simple in principle that it may be adopted by others who desire to carry this kind of experimentation further.

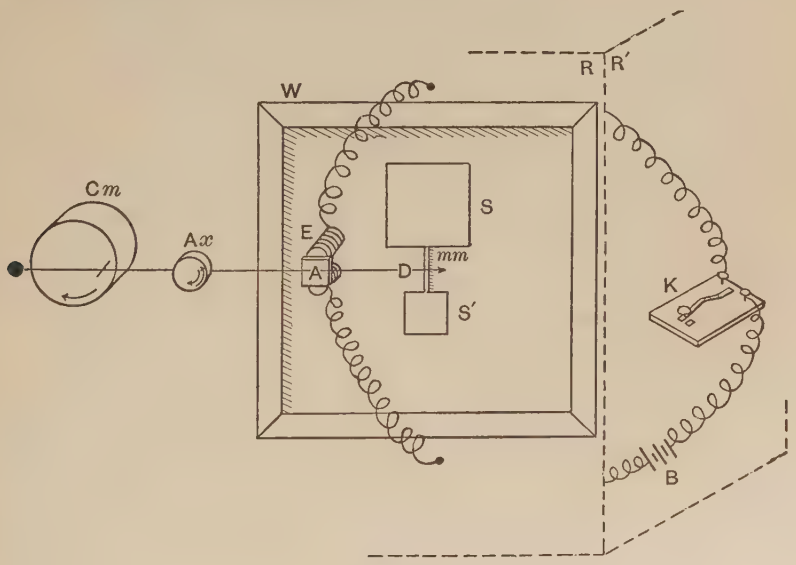


Fig. I.

The dark room (R) communicates with room I (R′) by a single window (W) which is completely filled with white

cardboard. In this cardboard two square holes are cut (S and S^1) whose sides are of determined ratio to each other, and whose distance from each other is measured by a slit (D) bearing a known ratio in length to the side of the larger square. On the wall beside the window (at Ax) is fixed the axis of movement of a long needle which is moved upon this axis by a pin carried round the face of the clock motor (Cm) of a Rothe polygraph. The movement of one end of the needle upward by the pin and downward by its own weight, is reversed by the other end of the needle, which so carries an arrow-head or pointed marker up and down the mm scale marked upon the slit D. The needle bears at A the armature of an electromagnet. The magnet (E) under the armature is fixed to the cardboard and its connections are carried into room R′ and terminate in a punch-key (K) on a table directly in front of the window W. The reagent sits at this key, closes the current when the needle reaches the mid-point of the slit D, the needle is arrested by the attraction of the magnet (E), and the reading is given on the scale mm. The apparatus works automatically, giving a series of experiments, with alternating up and down movement of the needle, until the motor runs down. A gas jet in room R is focussed through a large reading lens upon the scale mm, converting the small point of the needle seen by the reagent from the other room, into a moving bead of light. The back-ground of the squares and of the slit is the black of the dark-room wall, and the whole is seen by him upon the white surface of the cardboard.

For the horizontal arrangement of the squares, the whole apparatus is simply shifted 90°, bringing the axis of movement of the needle below the window.

With the arrangements thus described experiments were carried out on two persons; Sh., (W. J. Shaw) and T. (G. A. Tawney), with results as given in this report.[1] Both were practiced in psychological experimentation, but Sh. more than T.

[1] My thanks are due to these gentlemen, as also to two others who gave me some test series. For special reasons the conditions of reaction of the latter were not typical and so they were not continued.

In the case of each, the series of values of the ratio $\frac{S^1}{S}$ was $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, which gives, when S has the constant value of 20 cm, the following series of values for S^1, *i. e.*, 10, 5, 2.50, 1.25 cm. A constant value for the distance between the squares was selected which seemed about as likely to occur in ordinary arrangements and experiments as any other, *i. e.*, $\frac{1}{2}$ S = 10 cm.

The experiments were performed in series of 20 to 25, called each a 'lot,' only one lot being taken at a sitting to avoid fatigue of the eyes. The time of day was kept constant, the subject was kept in entire ignorance of the object of the research and of the results he gave, and was asked after each series to give any impressions he might have of the accuracy of his results, and of the variations which he made, if any, in his method of identifying the mid-point. Careful record was kept of all these impressions, and they turned out to be valuable.

Methods of Identifying the Mid-point.—The two reagents began at the very beginning of the experiments to describe their procedure differently—a difference which was persisted in and became in the sequel a matter of fundamental importance. Sh. tended to fix his gaze upon the moving bead of light; followed it in its course, and stopped it when it reached the mid-point. This, it is evident, involves an element of eye-movement through a series of positions corresponding in extent directly to half the time D. This I shall call the 'approach method,' since the mid-point is selected only as it is approached by the light-bead.

T., on the other hand, tended to select the mid-point first; and endeavored to hold it fixed until the light-bead reached it, then fixing the bead by his reaction. This evidently gives a result largely independent of eye-movements on the line D, and this may accordingly be called the 'fixation' method. It will be seen below that very important consequences follow from this difference of method.

I. Approach Method. Vertical Arrangement. Results of Sh. —The results of 770 experiments with the vertical arrangement upon Sh., who used the 'approach' method, divided

into 5 series of 6 lots each, are shown in Table I. In the 'vertical arrangement' the larger square was above the smaller in all cases. The variable error is not given in any of the tables, since it fell below the limit of accuracy of the apparatus, *i. e.*, the diameter of the light-bead. The uniformity in direction of the constant error is shown in the small number of exceptions or minus judgments given in the column Excpts. in the table. The words 'down,' 'up,' 'both,' signify the direction of movement of the needle.

TABLE I.—Sh. App. Method. Ver. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Down.	Up.	
155	20:10	2.35	2.7	2.	6
150	20:5	3.6	3.95	3.2	2
150	20:2.50	3.89	4.27	3.52	0
150	20:1.25	4.4	5.	3.8	0
165	20:0	2.8	2.93	2.66	1

The consideration of the figures given in this table enables us to formulate the following statements for the case in which the eye follows the stimulating bead to its point of arrest, up and down a vertical line:

1. There is a tendency to fix the mid-point too far away from the larger square (positive values of mean var.)

2. The direction of the tendency to error has practically no exceptions.

3. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other; *i. e.*, from .01215 of the side of the larger square when its ratio to the side of the smaller is 2:1, to .02 of the side of the larger when its ratio to the smaller is 16:1.

4. At the limiting value (0) of the side of the smaller square, the tendency to locate the mid-point too far away from the larger square is about the same as when the sides of the two squares are in the ratio 2 : 1.

5. The tendency to error is from 16 to 25 per cent. stronger when the stimulating object whose location is fixated is in movement in the same direction as the tendency of error (down), than when it is in movement in the opposite direction (up).

II. Results of Sh. Horizontal Arrangement.—Passing now to the horizontal arrangement, in which the details of apparatus remained the same as for the vertical, I may report as before for the two methods. The larger square was placed to the left, the smaller to the right, and the bead of light moved right and left over the slit between. The variations in the side of the smaller square gave as before the series of ratios to the side of the larger, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$. The following table shows the results:

TABLE II.—Sh. App. Method. Hor. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Right.	Left.	
100	20:5	.9	1.95	.33	20
50	20:2.5	1.67	2.5	.7	4
50	20:1.25	2.73	3.	2.46	2
50	20:0	2.1	2.	2.2	3

From the examination of Table II we gather the following results:

1. There is a practically uniform tendency of error away from the larger square.

2. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other.

3. The magnitude of the error is from .9 to 2.2 mm, *i. e.*, .005 to .01 of the side of the larger square.

4. At the limiting value (0) of the side of the small square the tendency is slightly less than when the ratio of the two sides is 16 : 1.

5. This tendency is about $\frac{1}{2}$ greater when the movement of the stimulus fixated is in the direction of the error itself (right) than when it is in the opposite direction (left).

III. Fixation Method. Vertical Arrangement. Results of T. —The results of 683 experiments with the vertical arrangement upon T., who used the fixation method, divided into five series of six lots each, are as follows. See Table III.

TABLE III.—T. Fix. Method. Ver. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Down.	Up.	
84	20:10.	2.96	1.99	3.85	4
150	20:5.	2.86	3.11	2.64	1
150	20:2.5	3.31	2.21	3.83	0
150	20:1.25	2.83	2.35	3.3	1
149	20:0	1.05	.8	1.35	21

Examination of this table enables us to make again the following statements for this subject with the method and arrangement described:

1. There is a tendency to error in the direction away from the larger square.

2. This tendency has so few exceptions that they are due probably to accidental causes.

3. The amount of this tendency is given in a number which fluctuates slightly about a value equal to .015 of the side of the larger square.

4. At the limiting value (0) of the side of the smaller square there is the same tendency to error, but it is less than $\frac{1}{2}$ the error when the ratio is 1 : 2.

5. The tendency to error is about 50 per cent. greater when the stimulus for fixation is moving in the direction contrary to that of the variation itself than when it is moving in the same direction.

IV. Results of T. Horizontal Arrangement.—The experiments on T. with the horizontal arrangement, his method remaining as before that which I have called the 'fixation method,' gave the results shown in Table IV.

TABLE IV.—T. Fix. Method. Hor. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Right.	Left.	
100	20:5	1.64	1.91	1.38	4
50	20:2.5	2.7	3.	2.5	0
50	20:1.25	3.25	3.65	2.9	0
25	20:0	2.6	1.53	3.9	0

From the examination of this table we may make the following statement of results for T.:

1. There is a uniform tendency to error in the direction away from the larger square.

2. This tendency is from 1.64 to 3.25 mm, *i. e.*, in this case .008 to .016 the side of the larger square.

3. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other.

4. At the limiting value (0) of the side of the smaller square the tendency to error is the same as when the ratio between the sides of the two squares is $\frac{1}{8}$.

5. The tendency is about $\frac{1}{3}$ greater when the stimulus fixated is moving in the direction of the tendency to error (right) than when it is moving in the opposite direction (up).

V. Rectification Method.—It is evident that a second series of indications may be obtained from the experiments given above in cases in which the reagent expresses his sense of the correctness or incorrectness of his result in each experiment. Both Sh. and T. were instructed to indicate after each experiment whether or not the bead gave a satisfactory result when stopped, and also in which direction the result should be rectified to give satisfaction. Records were kept of all such indications. Since it involved a secondary fixing of the mid-point, it approaches the 'fixation' method; but since it followed upon the earlier determination made when the needle was in motion, it involves influences akin to those of the 'approach' method; so it may be considered a combination of the earlier methods and a refinement upon both of them, for it requires a second act of judgment or criticism of the result already rendered in each trial. So let us call it the 'rectification' method.

It is further apparent that this rectification of the result of any given experiment may take one of four phases. It may be a judgment that the needle has gone too far, this we may call rectification by 'reversal;' or that it has not gone far enough, rectification by 'supplementing.' And each of these kinds of rectification will include again two instances. There will be reversals when the movement is in the direction of the prevailing error (*i. e.*, away from the larger square), and when the movement is contary to the direction of the prevailing error (*i. e.*, toward the greater square.) And the same two cases occur for the 'supplemental' rectifications.

The cases of rectification in the experiments on Sh. and T., both of whom were instructed to use the method, may be thrown into the following tables, in which the four kinds of rectification are distinguished.

Results for Sh. Rectification of Results Secured by Approach Method. Vertical Arrangement.—Giving the figures for Sh. in the vertical arrangement we have Table V.

TABLE V.—Sh. App. Method. Ver. Arrgt.

	No. Rects.	Ratio of Sides in cm.	Reversals.			Supplementals.		
			Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
	15	20:10	1	9	10	3	2	5
	14	20:5	1	7	8	5	1	6
	17	20:2.5	1	9	10	6	1	7
	33	20:1.25 / 20:0	5	18	23	6	4	10
Totals . .	79		8	43	51	20	8	28

From this table we may conclude as follows:

1. Of the rectification of results secured by the approach method, the 'reversals' are nearly twice as frequent as the 'supplementals.'

2. The 'reversals' are 5 times as frequent when the bead moves against the tendency to error as when it moves in the same direction.

3. The 'supplementals' are $2\frac{1}{2}$ times as frequent when the bead moves in the direction of the error as when it moves in the contrary direction.

4. Rectifications take place in $\frac{1}{10}$ the entire number of experiments.

Horizontal Arrangement.—The rectifications of Sh. for the horizontal arrangement are shown in Table VI (first line).

TABLE VI.—Hor. Arrgt.

Subject.	No. Rects.	Method.	Ratio of Sides in cm.	Reversals.			Supplementals.		
				Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
Sh.	38	App.	Whole series lumped.	8	24	32	3	3	6
T.	25	Fix.	"	12	7	19	1	5	6

It results from this table:

1. The 'reversals' number 5 times the 'supplementals' among the rectifications of data derived by the approach method.

2. The 'reversals' are 3 times as many when the bead moves in the direction contrary to the prevailing error (*i. e.*, toward the larger square), as when it moves in the opposite direction.

3. The supplementals are equally divided between the two cases of opposite movement of the bead.

4. The number of rectifications is about $\frac{1}{6}$ of the number of experiments.

Results for T. Rectifications of Results Secured by the Fixation Method. Vertical Arrangement.—The results of T. with the vertical arrangements appear in Table VII.

TABLE VII.—T. Fix. Method. Ver. Arrgt.

	No. Rects.	Ratio of Sides in cm.	Reversals.			Supplementals.		
			Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
	10	20:10			0	7	3	10
	19	20:5	12	5	17	1	1	2
	19	20:2.5	3	1	4	6	9	15
	31	20:1.25 / 20.0	7	9	16	4	11	15
Total . . .	79		22	15	37	18	24	42

Table VII shows the following:

1. Rectifications by 'supplementing' are $\frac{1}{7}$ more frequent than those by 'reversal' when the results are secured by the fixation method.

2. The 'reversals' are $\frac{1}{2}$ more frequent when the bead moves in the direction of error than when it moves in the contrary direction.

3. The 'supplementals' are $\frac{1}{3}$ more frequent when the bead moves in the direction contrary to that of the prevailing error than when it moves in the same direction as the error.

4. The entire number of rectifications is $\frac{1}{9}$ of the entire number of experiments.

Horizontal Arrangement.—The rectifications of T. for the horizontal arrangement are given in Table VII (second line).

1. *Results.*—The 'reversals' are three times the 'supplementals' in the fixation method, horizontal arrangement.

2. The reversals are $\frac{1}{2}$ more when the bead moves in the direction of error than when it moves in the opposite direction.

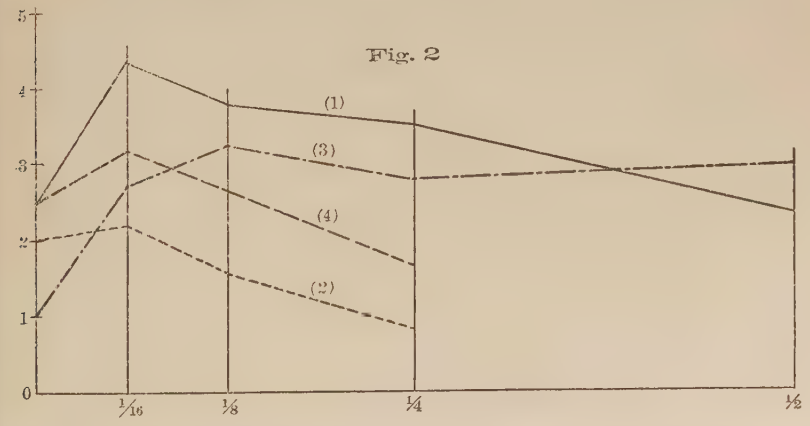
3. The 'supplementals' are five times more when the bead moves contrary to the direction of error than when it moves in the same direction. This result, however, is based on too small a number of cases to be taken as a numerical ratio.

4. The number of rectifications is $\frac{1}{9}$ of the whole number of experiments.

VI. General Interpretation of Results.—We are now able to gather up the results shown in the earlier tables in some more comprehensive statements, based upon the whole number of experiments taken together.

1. Considering the results for the direction and amount

Fig. 2



of error without regarding the influence of the direction of movement of the light-bead, we may plot curves showing

the tendency and amount of error for each of the two arrangements by each of the two methods. In Fig. 2 the horizontal ordinate represents the constant series of ratios of the square-sides to each other; the vertical ordinate, the size of the error and its duration (above the abscissa denoting error away from the larger square). Curves (1) and (2) give the results by the approach method, vertical and horizontal arrangements respectively; curves (3) and (4) the results by the fixation method, vertical and horizontal respectively. The location of the various points of the curves is determined in each instance by the figures given in the appropriate table above. The curves are numbered to correspond with the respective tables.

Inspection of the four curves gives certain general results which unite and summarize the results already shown from the separate tables above.

1. The four curves (representing 1,928 experiments) agree in establishing a tendency to error away from the larger square of from 1 to 4.5 mm when the side of the larger square is 20 cm.

2. The close parallelism of three of the curves in their common direction, and the general parallelism of all the four, establishes the fact that the tendency to error increases with the relative increase of the side of the larger square.

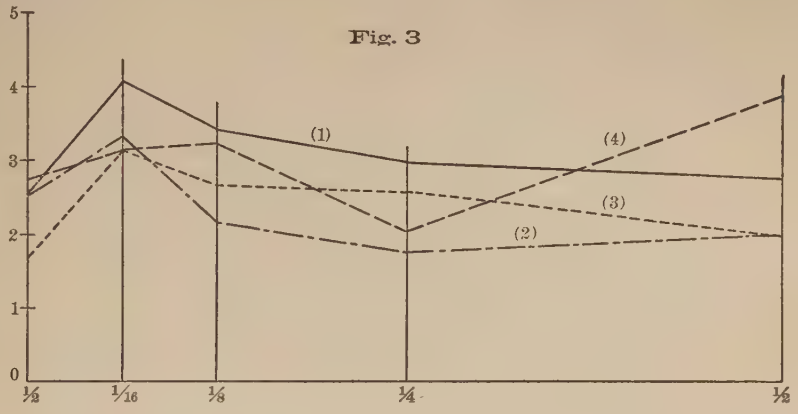
3. The position of curves (1) and (3), considered in relation to the position of curves (2) and (4), shows that the tendency to error, when the squares are arranged vertically, is about twice as great as when they are arranged horizontally.

4. Comparison of curves (1) and (2) with curves (3) and (4) shows that the method of fixation gives more uniform results than the method of approach; and also that the difference in result between the vertical and horizontal arrangements is less when the fixation method is used. It follows from this that eye-movements over a line hinder the correct estimate of the parts of that line, and that this influence of eye-movement is greater for vertical than for horizontal directions.

II. Considering the results with regard to the direction of movement of the light-bead by both methods and in both

arrangements, we may plot the curves of Fig. 3, in which the ordinates remain as in Fig. 2, the points on curves (1) and (2) give the amount of error for the several contrast ratios for the case of movements of the bead away from and toward the larger square respectively by the approach method, and the points on curves (3) and (4) give the amount of error for the same two cases respectively, by the fixation method. These amounts are reached by combining the figures for 'down' and 'right' movements in the tables of vertical and horizontal arrangement of the approach method, for each contrast ratio, and combining similarly the 'up' and 'left' results of the corresponding tables of the fixation method.

Fig. 3



Inspection of these four curves (again representing the entire 1,928 experiments) leads us to certain conclusions.

1. Comparison of curves (1) and (3) with curves (2) and (4) shows that the error is greater when the bead is moving in the direction of the error.

2. This is especially the case when the approach method is adopted, the error then being twice as great when the movement is in the direction of the normal error as when it is in the contrary direction [comparison of curves (1) and (2)].

3. It follows that the influence already found to be due to eye-movements varies according to the particular direction of the movement along the line explored. If the eye-movement is toward the larger of the areas contrasted, it tends to cor-

rect the normal error of judgment in the estimation of the time which connects the two areas. If the movement is, on the contrary, away from the greater area it exaggerates the normal error of judgment.

III. The details of the instances of 'rectification' given above serve to confirm these general conclusions, both as to the normal error itself and as to the influence of eye-movements upon it. By the approach method the rectifications by reversal are two to five times more frequent than those by supplementing. This shows that the rectifications in this instance are really corrections of the influences now found to be due to eye-movements. Further, reversals are three to four times as frequent when the bead moves against the tendency to error as when it moves in the direction of this tendency. This shows that these corrections are much more likely in direction opposite to that in which we now find the real contrast error to occur. When moving in the direction of the contrast error the eye-movement influence gets support from that error, and so fails of detection, and even secures supplementing in this direction more frequently than the movement in the opposite direction does. This is an indirect determination of the true direction of the contrast error in agreement with the direct experimental result.

The rectifications in the fixation method, on the other hand, are equally divided between the 'reversals' and the 'supplementals,' showing that the influence of eye-movement is largely eliminated by this method. And further, the distribution of both supplementals and reversals between the two cases of movement, in one direction or the other, is now directly reversed, *i. e.*, the reversals are more frequent when the bead moves in the direction of error, and the supplementals when it moves contrary to this direction, a result which seems to show that in this case the tendency to error from contrast is in conflict with the normal influence of eye-movements, and the correction is made to increase the latter in one direction, and to diminish the former (or their sum) in the other direction.

The entire number of rectifications of all kinds (about $\frac{1}{9}$ of the whole number of experiments) may be taken as a sort

of quantitative indication of the function of second-judgment, or deliberation, upon sensory determinations of such a complex character as those involved in these experiments. It is interesting to note that this second judgment, however, does not tend in the general result to correct the error of first judgment; for there are about ½ more cases of rectifications by displacement toward S^1 (the direction of the error) than toward S. The only case in which the correction does work to give greater accuracy to the result is that of the use of the fixation method, where both the original and second judgments are comparatively free from eye-movements and their after effects.

Finally, the great uniformity of the error of judgment is seen in the small number of cases (69 in the entire series of 1,928 experiments) in which the mid-point was located in the direction opposite to the prevailing error (that is, located too far toward the large square). And even this number represents too high a figure, since the sum of the variations of this kind in all but two series gave only 28 cases (*i. e.*, in 1,679 experiments); the two giving the very abnormally large figures 20 in 100 experiments (app. method, horiz. arrangement) and 21 in 149 experiments (fix. method, vert. arrangement) being evidently affected by some temporary influences.

A series of experiments has already been begun with a stationary stimulus (thus ruling out the influence of eye-movements); and I hope also to complicate the case with variations planned to introduce æsthetic elements into the problem.

IV. Types of Reaction.

BY J. MARK BALDWIN (with the assistance of W. J. Shaw.)

The experiments reported in this article were carried out in the Toronto Laboratory in 1892–93. Three questions were set for research, all of them bearing on the question of the degree of relativity of reaction times: either the difference of a single individual's times, according as there

were subjective (attention) or objective (qualitative stimulus) changes in the conditions of his reaction; or the differences of reaction times for different individuals under identical conditions. To secure results comparable in the respects in which comparisons were desired, certain precautions were made, as follows: (1) each reagent reacted at the same hour from day to day, and at the same hour with each other reagent whose reaction was to be compared with his; (2) the order of change in the conditions of reaction (as sensory-motor, light-dark, visual-kinæsthetic, etc.) was kept in the main the same for the different reagents.

The Hipp and D'Arsonval chronoscopes were used, both controlled by the records of a König tuning-fork recording on the drum of the Marey motor. The 'light' reactions were taken in a room of good south morning exposure, and those in the dark, in a dark closet of the same room. The stimulus was in all cases an auditory one—a sharp metallic click—and the reacting movement was a pressure downward of the right forefinger (in the case of the D'Arsonval instrument, a pinch of that finger and the thumb). The reagents were, besides the writers (B. and S.), Mr. Faircloth (F.), a student who had had only the experience gained from the practical work in this subject of the course in Experimental Psychology. His reactions were ready and unconfused, and from all appearances he was a normal and more than usually suitable man for such work. The fourth, Mr. Crawford (C.), is an honor student in this subject in Princeton. His reactions were taken in the course of another investigation, and being so few in number, they are included only because they give a certain case of a capable reagent whose sensory is shorter than his motor reaction. We hope to test him further.

I. *Variations in the Results.*—Table I. shows the relative reliability of the two instruments in these experiments.

TABLE I.—Clock-corrections.

Instrument.	Const. Error.	Var. Error.
D'Ars.,	.19	.06228
Hipp,	.019	.015616

All the results secured by each instrument are corrected, by the constant error of that instrument, before being used either for comparison among themselves or for compounding with the results of the other instrument, in the tables which follow. The smaller variable error of the Hipp chronoscope makes the results of that instrument much more reliable in the matter of absolute time-measurement. But in the conclusions drawn below, only those results are used in which the quanity sought is a relative one, and in which the two clocks confirmed each other in giving ratios of difference of the two quantities compared, both of which are in the same sense, and each of which is larger than the largest possible ratio of difference arising from the variable error of the clock to which it belongs.

The mean variations are not given in the tables which follow, because they are too complex to be of any value. These variations were different for the two clocks, as we would expect from the variable errors of the instruments themselves; they also varied with the disposition of the subject in the various groups of results which are treated together.[1] The different mean variations for the different lots of experiments varied from 10σ to 20σ. For this reason no deductions are attempted except those evident on the surface of the results themselves.

II. *Results: Sensory and Motor Reactions.*—Table II. gives the results of experiments on four persons designed to test the current distinction between 'sensory' and motor ('muscular') reactions.

[1] Finer distinctions were aimed at in some of the series, such as placing the sound stimulus on one side only, or in the median plane below the head, etc., as well as arranging for the difference between light and dark environment.

TABLE II.—Types of Reaction.

Subject.	No. Exps.	Sensory.		Kin. Motor.		Vis. Motor.		Av. Motor.	
		No.	Time in σ.	No.	Time in σ.	No.	Time in σ.	No.	Time.
B.	2490	1043	178	966	149	481	171		160
S.	2572	1017	235	995	184	560	195		187
F.	820	290	164	260	202.3	270	205.5		204
C.	212	84	132					128	157

It follows from Table II.: (1) that the current distinction between sensory and motor reactions does not hold in the sense that the motor reaction is always shorter than the sensory, for in the case of F. the motor reaction is 40 σ longer, *i. e.*, $\frac{1}{5}$ of this subject's average sensory reaction time. (2) As between B. and S., in the case of each of whom the motor-time is shorter, there is a great difference in the relative length of the sensory to the motor. In B. the sensory time is only 18 σ, or about $\frac{1}{9}$ longer than the motor, while in the case of S. the sensory is 48 σ longer, or about $\frac{1}{4}$; and this despite the close agreement of the two subjects in their absolute motor-time. We would seem to have, therefore, in these three observers three cases shown, two giving very pronounced results; one a longer motor time by $\frac{1}{3}$, and the other a longer sensor by $\frac{1}{3}$. The third subject, B., seems to fall between these extremes, giving a difference in favor of the motor reaction, it is true, but a much smaller difference.

The tables also give us reason for accepting the truth of the distinction between two kinds of motor reaction. In both B. and S., whose motor reactions are shorter than the sensory, we find a difference in the length of the motor reaction according as the attention is given to the movement by thought of the hand, the eyes being blindfolded; or as the attention is fixed upon the hand, which is seen. The former I have called the *kinæsthetic motor* reaction, the latter

the *visual motor*. In B. the visual motor is 22σ, or about $\frac{1}{7}$ longer than the 'kinæsthetic'—that is, it is practically equal to this subject's sensory time; while in S. the kinæsthetic motor is 11σ shorter than the 'visual.' With F., on the contrary, there is no distinction between the two kinds, any possible trace of it seeming to be lost in the excessive preponderance, in facility, of the sensory kind of reaction.

The table as a whole, then, supports the views: (1) when the motor reaction is short in relation to the sensory (case of S.), then this motor reaction is purest, freest from sensory influences, such as sight, etc.; (2) when the motor reaction is not pure, then it is retarded by such influences as sight (case of B.); (3) where the motor reaction is relatively difficult and delayed, as compared with the sensory (case of F.), there this prime difference renders all kinds of motor reactions equally lengthy and hesitating. B. seems to stand midway between the two others in this respect.

As I said some time ago, in making a first report upon the outcome of some of these experiments:[1] "In subjects of the motor type the 'kinæsthetic motor' is shorter, the 'visual motor' time approximating the sensory reaction time."

III. *Light and Dark Reactions to Sound.*—The foregoing deductions concerning the difference between B. and S., as respects motor and sensory reactions, and also as respects the distinction between visual and kinæsthetic motor reactions, are confirmed by results of a research on the same two subjects, in which the attempt was made to investigate the influence of vision. Each reagent gave a series of reactions in the light of an ordinary laboratory room, and then repeated the series under the same general conditions, but in a dark chamber. In this case, in order to make the results of the two series comparable, the kinæsthetic form of motor reaction was necessary in the series taken in the light, since only that kind of motor reaction was possible in the dark. The results are given in Table III.

[1] New York *Medical Record*, April 15, 1893, p. 455 f.

TABLE III.—Reactions in Light and Dark.

SUBJECT.	LIGHT.				DARK.			
	Sensory.		Motor.		Sensory.		Motor.	
	No.	Av. in σ.	No.	Av. in σ.	No.	Av. in σ.	No.	Av. in σ.
B.	541	176	979	164	502	184	468	138
S.	537	237	1190	158	480	219	469	179

On examination the data of this table, compared with those of the preceding table, may be stated as follows: We find for B. that the sensory reaction is practically the same, whether he react in the dark or in the light (the latter is less by 8σ, which is insignificant in view of the variable error). This shows this subject's independence of vision in the sensory reaction to auditory stimulations, and is in agreement with the results of Table II. (in which there is a similar difference between the sensory and visual motor, the former being longer by 7σ). S., on the other hand, shows a shortening of the sensory reaction when in the dark by 18σ, but a lengthening of the motor reaction when in the dark by 21σ, or about $\frac{1}{7}$. The latter result shows this subject's dependence upon vision only in the motor kind of reaction.[1]

IV. *Interpretation.*—Admitting that these results indicate clearly the existence of persons whose sensory reactions to sound are shorter than their motor reactions, and that there is in some individuals a difference between the length of the motor reaction, according as it is made in the light or in the dark, we may make some general remarks on the theory of these differences. These results should be compared with earlier ones, a matter which is made easier by reference to the concise summing up of the literature of the subject by

[1] The 'dark-reaction' was not secured from F., the 'sensory' subject; but we hope to report further results obtained from C., the similar case now found in Princeton.

Titchener in *Mind*.[1] We find cases of relatively shorter sensory times similar to mine reported (for electrical stimulus) by Cattell,[2] and (for sound stimulus) by Flournoy[3]. We may accordingly say that such individual differences are clearly established, and must hereafter be acknowledged and accounted for in any adequate theory of reaction.

The attempt of Wundt, Külpe and others to rule these results out, on the ground of incompetency in the reagents, is in my opinion a flagrant *argumentum in circulo*. Their contention is that a certain mental *Anlage* or aptitude is necessary in order to experimentation on reaction-times. And when we ask what the *Anlage* is, we are told that the only indication of it is the ability of the reagent to turn out reactions which give the distinction between motor and sensory time, which Wundt and his followers consider the proper one. In other words, only certain cases prove their result, and these cases are selected because they prove that result. It is easy to see that this manner of procedure is subversive both of scientific method and of safely-acquired results in individual psychology. For the question comes: what of these very differences of individual *Anlage?* How did they arise; what do they mean; why do they give different reaction-time results? To neglect these questions, and rule out all *Anlagen* but one, is to get the psychology of some individuals and force it upon others, and thus to make the reaction-method of investigation simply the handmaid to dogma.

The attempts to explain the relative shortness of the 'muscular' reaction, also, by those who hold its shortness to be a universal fact, have been unfortunate. It has been

[1] Jan., 1895, p. 74.

[2] *Philos. Studien*, VIII., 403.

[3] *Arch. des Sci. Phy. et Nat.*, XXVII., p. 575, and XXVIII., p. 319. Titchener, in his summing up, does not cite the cases of Flournoy nor the earlier report of one of my present cases (F.) in the *Medical Record*, Apl. 15, 1893, although they tell directly against his own views. My earliest case was noted by me in the autumn of 1892, and the note in the *Medical Record* was written in December, 1892, before I saw either Cattell's or Flournoy's articles. The sentences quoted from my *Senses and Intellect* by Titchener in *Mind*, *loc. cit.*, were based upon my own reaction-times, taken earlier when I had no reason to doubt the universality of the experience, as claimed by Lange and Wundt. Titchener is accordingly wrong in citing me as favoring that position.

held that the muscular reaction is shorter because it is semi-automatic; the thought of a movement, *i. e.*, attention to it, being already the beginning of the innervations necessary to its production. This is very true as a principle, I think; but it is just the application of this principle which makes it necessary on the part of some to restrict reaction work to people of a special aptitude. For in all those cases, either of particular reactions in one individual or of all reactions in other individuals, in which the movement is not so clearly picturable as to be firmly and steadily held in the attention, to these cases the principle does not apply. On the contrary, to all cases where it is difficult to get the attention fixed upon a *motor* representative of the movement, a very different principle applies, as Prof. Cattell has said. The very attempt to picture a movement *as a movement*—by putting the attention on its *motor* aspect in consciousness—embarrasses, confuses and delays the execution of that movement in these cases. If a marksman attend to his finger on the trigger he misses the target; if a ball-player attend to his hands he 'muffs' the ball; if a musician think of each finger-movement he breaks down. The musician in the laboratory is usually, indeed, a glaring instance of unsuitable *Anlage!*

So it is evident that these two principles need reconciling in their application to reaction-times, the principles, *i. e.*, (1), that *the thought of a movement already begins it, facilitates it, quickens it;* and (2) *that attention to a practised movement, in many instances, embarrasses it, hinders it, lengthens it.*

Now the practical reconciliation of just these two principles has been made in another great department of fact, and the actual plotting done of the cerebral arrangements which underlie them—a solution which has such evident application here that I wonder at its tardy appreciation. I refer to the work in the pathology of aphasia, and the general theory of mental 'types' which now goes for a safe discovery in the discussions of 'internal speech,' 'sensory *vs.* motor defects' of speech, etc. I published early in 1893[1] an hy-

[1] See the *Medical Record* (N. Y.), *loc. cit.*

pothesis to account for the variations in this matter of reaction-time differences, in these words:

"I have endeavored incidentally, in an article now in print for the July issue of the *Philosophical Review*,[1] to account for the conflicting results of experiment in this field, by borrowing from the medical psychologists the results of their brilliant analysis of the speech function, on the basis of its pathology. The recognition of the great forms of aphasia—*i. e.*, sensory and motor—and the corresponding recognition of the existence of visual, auditory, and motor speech types, gives a strong presumption that the distinction between sensory and motor in the voluntary movements of speech and writing applies as well to voluntary movements of all kinds; that is, to all movements which have been learned by attention and effort. This means that a man is an 'auditive,' or a 'visual,' or a 'motor' in his voluntary movements generally. His attention is trained by habit, education, etc., more upon one class of images than upon others, his mind fills up more easily with images of this class, and his mental processes and voluntary reactions proceed by preference along these channels of easiest function.

"If this be true, it is evident that a man's reaction-time will show the influence of his memory type. The motor-reaction we should expect to be most abbreviated in the man of the motor type; and less abbreviated, or not so at all, in the 'visual' or 'auditory' man. And experimental results must perforce show extraordinary variations as long as these typical varieties are not taken account of. We are accordingly, I think, a long way off from any such exact statement of absolute difference between sensory and motor reaction-time as Wundt makes in his last edition."[2]

It was a sense of the great naturalness and probability of this hypothesis that led me early in the fall of 1892 to institute the experiments on 'visual' and 'kinæsthetic' motor reactions whose results are given above in this paper.[3]

The secure establishing of cases which show sensory reactions shorter than motor (*i. e.*, the cases now reported by Cattell, Flournoy and myself), together with the probable

[1] Article entitled "Internal Speech and Song," *Phil. Rev.*, July, 1893.

[2] *Physiologische Psychologie*, 3d ed., II., p. 261 ff.

[3] At the Philadelphia meeting of the American Psychological Association, on Dec. 28, 1892, I proposed the hypothesis informally to several American psychologists. Dr. Lightner Witmer will remember a conversation in which the point was remarked upon. I venture to make these personal explanations since a somewhat similar explanation of his cases was advanced by Prof. Flournoy, of Geneva, in the articles cited above. I was not acquainted with Prof. Flournoy's views until, a year later at the New York meeting of the Association, Prof. Cattell brought them to my attention, as given in abstracts in the *Revue Philosophique* and the *Zeit. für Psych.* I return to Flournoy's position later on in this paper.

distinction between the 'visual' and 'kinæsthetic' forms of motor time, make it advisable that this hypothesis should be put in clearer evidence. I shall therefore proceed to state the case for it briefly on the basis of the facts as they are now known.

The doctrine of 'types' rests upon certain facts which may be briefly summed up. A voluntary motor performance—say speech—depends in each particular exercise of it, upon the possibility of getting clearly in mind (*intérieur*, *innerlich*) some mental picture, image, presentation, which has come to stand for or represent the particular movements involved. This mental 'cue' or representative may belong to either of two great classes: it may be a 'sensory' cue or a 'motor' cue. People are of the sensory type or of the motor type for speech according as their cue in speech is sensory or motor; that is, according as in speaking they think of the sounds of the words as heard, the look of the words as written, etc.—the cues furnished by the special senses associated habitually with speech—this on the one hand; or according as, on the other hand, they think of or have in mind the movements of the vocal organs, lips, tongue, etc., involved in speech. In the 'motor' people there are incipient movements in mind; in the 'sensory' people there are special sense images in mind. All this is now so clear from the pathological cases examined that the theory of localization of brain areas and their connections is applied to the successful exploration of damages of the brain when aphasic symptoms furnish the main diagnostic resource.

Now, let us see how in these cases of aphasia the two principles spoken of are applied. Suppose we agree with the neurologists in saying that the function of the 'cue'—the mental image, be it either motor or sensory, which when thought of enables a man to speak—is to release energy from its own brain-seat, along association fibres or pathways, to the motor-seat which sends its discharges out to bring about the movement. Then the difference between sensory and motor people is simply that different centres—different 'cue'-seats—have these connections with the motor speech centres best or better developed. A man who speaks best

when he thinks of the sounds of the words has his best 'cue'-seat in the auditory centre; and his best pathway to the speech motor-centre goes out from this 'cue'-seat. For the man who speaks best when he thinks of the utterance of words, the same may be said of the muscle-sense seat.

So it is evident—apart quite from the question as to how one or other state of things comes to be as it is in any one case—that with one man attention is directed to the *movement* for the best results, with another to the sensation or special memory image in best association with the movement. With the former *the thought of the movement begins the movement*. But with the other, if the best doing of the movement comes from thinking of a sensation or special image, *then the movement will be relatively deranged, embarrassed, when the attention is drawn from this sensation* and forced to fix itself upon the movement itself. These, then, are the two principles we desiderate, and they are both natural parts of the 'type' theory.

So why not generalize this? Speech cannot be considered an exceptional function in its rise and mechanism. Other complex motor functions show the same kinds or types of execution: handwriting, music performing, etc.[1] The hand has, next to the tongue possibly, the most delicate, varied and differentiated functions to perform; and the laws of association by which sensory cues, checks, controls, are affixed to hand actions and combinations, must be the same as those involved in speech. So in simple hand movements people must show the sensory and motor types. This is my hypothesis.

The man, therefore, who gives relatively shorter motor reactions is a 'motor' in his type; with him the thought of movement is the most facile beginning of the movement, just because it is *really the movement*, and nothing else, that he thinks of. That is his *Anlage*. But the man who gives relatively shorter sensory (auditory, visual) reactions, is a 'sensory' in his type: with him the attempt to think of the

[1] See my *Mental Development: Methods and Processes*, pp. 91 ff., and 438 ff. In Chap. XIV. of that work, on 'The Mechanism of Revival,' I have endeavored to put in evidence the general principles which underlie the type theory.

movement *as a movement* interferes with the prompt and exact execution of it, just because he is not accustomed to execute his movements in that way. That is his *Anlage*. But, of course, the two sorts of people have equal claim to recognition in science. Suppose a dead aphasic brought for autopsy to a surgeon, who inquires into the life-history of the man, and finding that he was of the sensory type, then declares that the body is not fit for a scientific autopsy, because the man did not have the proper type of aphasia! As a matter of fact, so near are the disciples of Wundt to the explanation by types that it is only necessary to translate their word *Anlage* by 'type,' and then apply the connotations of that term in the examination of refractory cases, to bring them into line. I may accordingly sum up in the words of my earlier article (*Philos. Rev.*, II, 395):

"We have in this fact of types the explanation of the contradictory results reached by different investigators in the matter of motor reactions. Some find motor reactions shorter, as I have said above; others do not. The reason is, probably, that in some subjects the 'sensory' type is so pronounced that the attention cannot be held on the muscular reaction without giving confusion and an abortive result. On the other hand, some persons are so clearly 'motor' in ordinary life, that sensory reaction is in like manner artificial, and its time correspondingly long. And yet again others may be neutral as regards sensor or motor preferences. If this be true, another element of 'abounding uncertainty' is introduced into all the results of experiments so far performed in this field, as reflection on the matter will show."

One or two further points, however, may be made which give the correct interpretation more importance than the simple facts in themselves really have. In the first place, an additional tendency seems to show itself when movements become very habitual—a tendency recognized in all discussions of the principle of habit. Habitual performances tend to become independent of consciousness, attention, thought, altogether. This tendency should make itself evident in reaction-time work, and reagents of great practise should show, (1) diminishing time in all reactions, and (2) diminishing difference between the two kinds of times, sensory and muscular. Further, the same tendency should show itself in a diminishing difference between individuals

of different types as they both get more practise. All these results are, I think, clearly shown in those of the earlier researches in which the amount of practise is reported.[1]

And, again, finer distinctions of type follow from the general theory: such distinctions as those clearly established for speech. The 'visual,' 'auditory,' and possibly (as in the blind) 'touch' types of speech are all included under the head of sensory. As I have said, the speech case *is* a case of finer reaction-time distinctions. And the hand, as used in most reaction experiments, ought to show to a greater or less degree similar distinctions.[2] The cases so far discovered of relatively shorter sensory reactions seem to be, as far as reported, auditory (musicians) and visual (Flournoy's). To determine between 'visual' and 'auditory' times for any individual, of course the same set of reaction experiments should be made with the two classes of stimulations, each being compared with the muscular reactions to the same stimulus respectively.

The general result follows (if this hypothesis get acceptance) that the reaction-time experiment becomes of use mainly as a *method*. Distinctions supposed to be established once for all by various researches must be considered as largely individual results, inasmuch as the authors have not reported on the type of the reagent. But for that very reason these results may have great value, as themselves indicating in each case this very thing, the type of one single reagent, and in so far some of the general characteristics of that type. What we now desiderate in a great many departments, as, for example, in the treatment of school children, and in the diagnosis of complex mental troubles, is just some method of discovering the type of the individual

[1] Consequently it does not do to say, with Wundt and Külpe, that the 'muscular' reaction is more automatic. Of course it is so in those who give a shorter motor reaction—that is sufficient proof of it. But that the sensory time is shorter in others is sufficient proof, also, that in their case the sensory reaction is more automatic. Külpe's two-arm reaction experiment is subject to this criticism, among others (see Wundt, *loc. cit.*, p. 325; Külpe's *Grundriss*, pp. 422 f.).

[2] A possible instance of such variation is seen in the case of Donders, which Wundt has difficulty with (*Phys. Psych.*, III^e., II., p. 268). Say the reagent was 'visual' in his type, and we have reason for his shorter reactionto light than to sound, while he still falls under the sensory type in general.

in hand. If reactions vary in certain great ways, according to the types which they illustrate, then in reaction experimentation we have a great objective method of study. But before the method can be called in any way complete, there should be a detailed re-investigation of the whole question, with a view to the great distinctions of mental type already made out by the pathologists.[1]

A word should be added concerning the position of Professor Flournoy. The hypothesis which I have advanced has been attributed also to Flournoy, his name being connected with mine as independent advocates of it (by Cattell, Titchener, etc.). I think this is a mistake, at least so far as the publications of Professor Flournoy are taken as evidence. His case, cited as of the 'type visuel,' seems to imply the existence of other types it is true; and at the close of his second article he raises the question, "si la façon de reagir observée chez M. Y., n'est qu'une singularité individuelle, ou si elle est un fait général et constant dans le type visuel d'imagination." But what he means in the context by 'type visuel' is not what is meant by that phrase in the generalized usage of the pathologists. His case is 'visual' in the sense that the man thinks of movements by a visual picture of his arm, rather than by muscle-sense images (just what I have distinguished above as 'visual motor' in distinction from 'kinæsthetic motor;' and the case is a fine confirmation of the conclusions given above under that head). But it does not follow that the man is a 'visual' in the broader sense. He might just as likely be an 'auditive.' The most that can be said of Flournoy's case, on the general doctrine of types—other evidence aside—is that he is 'sensory,' and on my theory his shorter sensory reaction-time proves it. But Flournoy makes no such general application of the theory of types. Indeed, in asking the question which I have quoted from him (*i. e.*, whether all visuals would react as this man did), he shows that he does not mean

[1] I have earlier indicated (*Med. Record*, *loc. cit.*, and *Philos. Rev.*, *loc. cit.*), the possible use of this method by brain surgeons, an idea which Wallaschek comments on with approval. Certain general indications from reaction-times are already recognized by physicians, especially in investigating various anæsthesias.

to bring reactions generally under the type theory. For the real 'visual' might give a shorter 'visual motor' than 'sensory' time—*i. e.*, when the stimulus reacted to is other than visual (say auditory); since then the *visualizing habit* would throw its influence on the side of the motor reaction.

In the matter of the distinction between 'visual motor' and 'kinæsthetic motor' reactions, however, Flournoy's case clearly anticipated mine in print.[1]

V. Sensations of Rotation.

BY H. C. WARREN.

The following experiments were undertaken in connection with some other series, with a view to determining the manner in which conflicting data from different senses are harmonized. As they have, in addition to this, a special bearing on the sense of rotation, it seems best to give their results separately. The particular object of this investigation was to determine the relative influence of sight and the internal sense of rotation on the subjective estimate of movement. Messrs. W. J. Shaw and G. A. Tawney kindly acted as subjects during the entire series, which consisted of sittings about 20 minutes long, twice a week, extending over a period of four months in the early part of the year 1894.

The experiments were conducted in a dark room, 8x8 ft. The subject lay at full length on his back on a rotation-board, with the head somewhat raised and eyes so screened as to permit of his seeing only a small area of wall in front. At the foot of the board, and covering entirely his restricted field of vision, was a large screen with an aperture eight inches square, behind which a mirror could be hung. On two opposite walls of the room were hung strips of white paper, an inch wide, reaching nearly from ceiling to floor, and placed about a foot apart, which could be seen plainly in a very faint light, when no other outlines or shadows were

[1] Since revising the proofs of this article I have received a note from M. Flournoy in which he says: "Je suis, d'une façon générale, d'accord avec vous sur l'influence du type d'imagination" (making reference to my article in the *Medical Record*).

distinguishable on the screen or wall. When the subject closed his eyes, or the board was turned so that no strips were visible, the usual phenomena of rotary sensation were observed; the least discernible movement was 1° a second; a movement greater than this, if continued, gradually ceased to be noticeable, and any change in the rate was then interpreted as a new movement starting from a state of rest. But when the subject, believing himself to be at rest, was turned so that the strips came into view, a conflict arose between the internal sense and the visual sense, which had to be reconciled by some mode of interpreting the data. The experiments included trials both with and without the mirror, the subject never knowing whether the mirror was in or not.[1]

(1) With the minimum light by which the strips were discernible, the sense of sight played no rôle whatever in the judgment, the strips appearing sometimes to flit across the field of vision, and sometimes to move with the subject, according to the data furnished by the internal sense. (2) With an illumination ranging from one to six candles, visual impressions strengthened the internal sense of movement when they agreed, or tended to inhibit it when the conflict was not too great. Thus, with the mirror removed, the sight of the strips made even the slightest movement perceptible, and checked the sensation of reversed movement which occurs after a real movement ceases. With the mirror in, the least perceptible movement was between 1° and 2° per second. Movements greater than this were usually interpreted (when the strips were visible) as lateral movements of *progression in the direction in which the head was actually moving*. A sudden stop converted this into a judgment of *rotation in the opposite direction*. Sometimes, however, especially with very rapid movements, they were

[1] One of the subjects (T.) did not know of the mirror at all until the series was nearly completed. S. generally could not tell whether it was in or not; I questioned him at the end of each sitting, and found that, as a matter of fact, he never considered it in making his judgments, being too busy observing and reporting. T. was more inclined to dizziness than S., and his experiences were consequently less distinct and his answers less uniform. In general the two subjects agreed. I also confirmed a number of the results here given by experiments on myself.

interpreted as rotary, and the strips were declared to be moving also, but faster than the subject. (3) With a bright gas jet burning, the corners of the walls and many other outlines were plainly visible, and gave more general indications of a stable environment than the strips afforded. The judgment of *progressive movement* now occurred uniformly, except when the board was started with a sudden jar; the subject was unable to rid himself of the illusion; he would seem to be moving steadily sideways, even though he knew the impossibility of doing so in a small room. The writer confirmed these results personally. (4) In the final series the subject sat upright at the center of the board, with his head slightly in front of the center of rotation. In this position the interpretation of the movement as progressive is impossible, if the internal sensations in the head are regarded; and as a matter of fact no such judgment was given. Careful and repeated measurement by the metronome gave the following results, with the mirror in and a very strong light (two gas-jets): (*a*) Movements of less than 2° per second were judged to be objective merely. (*b*) Movements of 2°–5° per second were interpreted as subjective, but in the *reverse direction* from that in which they were actually occurring; *i. e.*, they were felt as movements, but their direction was determined not by the internal sense, but by sight, and they were thought to be in the opposite direction on account of the mirror. (*c*) Movements greater than 5° per second were interpreted correctly, with the remark that the wall was moving also in the same direction as the feet, and faster. The transition from (*b*) to (*c*) was very distinct; several times the speed was varied during the trial from greater to less than 5°, or vice versa, and each time there was an immediate change of judgment as to the direction of the movement. A single 'trial' in this case was always limited to 10 sec., in order to avoid the feeling of 'slowing up' which accompanies uniform movement.

Aside from the last-mentioned phenomenon, which belongs more properly to the general subject of conflict among sense impressions, the most noteworthy result was the transformation of rotary into progressive movement, by means of

visual data. Suppose the actual movement to be clock-wise. The head moves to the left, the feet to the right, and the strips and wall reflected beyond the feet are carried much faster to the right. But only the difference between the rate of the feet and the rate of the back-ground is taken into account; the back-ground is considered stationary, and the movement is interpreted as a movement of the subject bodily to the left. We may infer from this that the end-organ of the internal sense of rotation is in the head alone, since movements of the lower extremities are open to such absolute misjudgment. We are also led to the conclusion that the organ for the sense of rotation is the same as that for progressive movement. This is directly contrary to the conclusion reached by Delage, who denies that the sense-organ for progressive movement is in the head, while admitting it for rotary movement.[1] These results, moreover, seem to favor the view that the semi-circular canals constitute that organ, in spite of the objections recently raised by Ayers and others on morphological grounds.[2] A special sense-organ seems requisite for such a sense, and the above results tend to locate that organ in the head. In our experiments the sense of sight was made to furnish data of movement independent of the internal sense. In the *head* the latter sense was so strong that, within the given limits of visual field and luminosity, a conflict between the two senses invariably resulted in its favor, and any movement observed merely visually was attributed to the objects in the environment. Yet when the *lower extremities* were moving quite rapidly in one direction they were constantly declared to be moving in the other, on the testimony of visual data. The 'internal sense' of movement must therefore be due to something other than the general indication furnished by the vaso-motor system, which would affect all parts of the body alike.

[1] Physiologische Studien über die Orientirung, von Delage; deutsch von H. Aubert; pp. 94–95.

[2] Journal of Morphol. 1892, VI, pp. 237–256.

SHORTER CONTRIBUTIONS.

The 'Haunted Swing' Illusion.

BY R. W. WOOD.

I was much interested this summer in the curious sensations produced by a purely optical illusion, known as the 'Haunted Swing,' at the Midwinter Fair in San Francisco. On entering the building we found ourselves in a spacious cubical room, furnished with a sofa, table, chairs, etc., a massive iron safe, and a piano, together with other minor articles. But the most conspicuous object was the huge swing, capable of holding forty or more persons, which hung in the centre, suspended from an iron cylinder which passed through the centre of the room. We took our seats and the swing was put in motion, the arc gradually increasing in amplitude until each oscillation carried us apparently into the upper corners of the room. Each vibration of the swing caused those peculiar 'empty' sensations within which one feels in an elevator; and as we rushed backwards toward the top of the room there was a distinct feeling of 'leaning forward,' if I can so describe it—such as one always experiences in a backward swing, and an involuntary clutching at the seats to keep from being pitched out. We were then told to hold on tightly as the swing was going clear over, and, sure enough, so it did, though the illusion was not so perfect as the high oscillations.

The device was worked in the following way: The swing proper was practically at rest, merely being joggled a trifle, while the room itself was put in motion, the furniture being fastened down to the floor, so that it could be turned completely over. The illusion was good, though the absence of centrifugal force, and the fact that the swing did not move with uniform acceleration as it descended, would indicate to a careful observer that he was not swinging freely. The curious and interesting feature however, was, that even though the action was fully understood, as it was in my case, it was impossible to quench the sensations of 'goneness within' with each apparent rush of the swing. The minute the eyes were shut the sensations vanished instantly. Many persons were actually made sick by the illusion. I have met

a number of gentlemen who said they could scarcely walk out of the building from dizziness and nausea. I myself experienced no sensations of dizziness, being accustomed to heights and to rapid motion; but the sensation before described was always present (and I visited the place several times), though I tried to suppress it and reason against it.

Heat Sensations in the Teeth.

BY HENRY RUTGERS MARSHALL.

In the course of a late operation upon one of my teeth I experienced a very powerful and distinct sensation of heat whenever the dental instrument touched a very thin layer of the tooth substance (dentine) which still remained protecting the 'pulp' from exposure.

The well-known Dr. Frank Abbott, who operated upon my tooth, and whose long and wide experience enables him to speak with authority, assures me that this sensation of heat is entirely independent of the temperature of the instrument employed: that in his experience he finds that any mechanical irritation of the dentinal fibers, when inflamed, will produce this sensation of burning, it being especially marked when the fibres are dragged asunder by the revolving instruments often used. The same heat sensation is produced, he tells me, by the rapid absorption of moisture produced by placing against this highly sensitive tissue a bit of 'spunk' or 'bulbulous paper,' or other rapid-absorbing substance. The substance called 'spunk,' which he uses for this purpose, is supposed to be nothing more than a tree fungus of especially fine fibrous nature.

It is apparent that we have here a production of heat sensations by stimulations which do not correspond in any evident way with the stimulations that produce heat in the 'heat spots' on the surface of the skin. I think it well to make note of these particular dental experiences in order that those who may be investigating the nature of the processes involved in the production of our sensations of heat and cold may upon occasion verify them, and may coördinate them with the more familiar means of heat production in the formulation of their theories.

DISCUSSION.

RECENT DEVELOPMENTS IN THEORY OF EMOTION.

Considerable interest attaches to the recent articles on Emotion by Professors Baldwin and Dewey. Both these writers are in favor of the James theory, but each has arrived at his results in his own way, and has his own view with regard to the weak points of the original statement of the theory and the value of the emendations that have recently been made. I do not propose, however, to deal with all the points of interest here involved. I shall merely discuss briefly the main arguments brought forward.

Professor Dewey contends that "all so-called expressions of emotion are in reality the reduction of movements and stimulations originally useful into attitudes" (Psy. Rev., I, 6, 569). They are explicable by reference to useful acts, and therefore cannot be deduced from an antecedent emotion. Now, the term 'expression of emotion' is ambiguous, not only in denotation, but also in connotation. Few would assert that the bodily changes usually signified by this mode of speech are primarily intended to show that a certain individual has a particular emotion. The majority of psychologists would agree that the movements in question are in the main teleologically conditioned. The question is, whether you can conclude from this that they cannot be regarded as the outcome of some antecedent emotion. Professor Dewey's argument depends on the assumption that no useful action can be explained by reference to emotion. But surely the natural supposition is that so prominent a fact of consciousness has some function, and therefore directly or indirectly influences our actions. Professor Dewey himself says that "hope, fear, delight, sorrow, terror, love are too important and too relevant in our lives to be in the main the 'feel' of bodily attitudes which have themselves no meaning" (Psy. Rev., I, 6, 563). It is hard to see how these can be important or relevant in our lives, if it is enough to dissociate an action from them to show that it has a purpose or end. I do not assert that all the bodily changes, internal and external, are the result of emotion. My contention is that it does not follow, merely because movements are

purposive, that they have no connection with emotion; and, further, that it would be a strange thing if this conclusion were correct.

We must now consider how far the detailed account of the bodily changes supports the 'effect' theory. A distinction is drawn, implicitly at all events, between the internal organic disturbance and those outward movements which are actions in the true sense, since they are directed towards some end. With regard to the latter, no explanation is given. All that is done is to show that they are directly teleological. It is first assumed that emotion can have no connection with useful actions, and then actions of this sort are simply taken for granted. If, however, they cannot be deduced from the emotion, it is legitimate to demand some account of their origin. It cannot be too strongly emphasized that the so-called 'cause' theory has an intelligible explanation to offer, while the 'effect' theory is silent on the point. Further, as Professor Baldwin brings out so clearly, it is not easy to see how any explanation can be given, on the principles of the latter view, which will fit into a theory of development.

A complication is introduced by the fact that some of these movements, such as the crouching of fear and the clenching of the fist in anger, are reflex. It is easy to understand, however, that once these actions have been voluntarily performed under the influence of a certain emotion, they arise reflexly in circumstances similar to those with which they were first connected. The identity of circumstance redintegrates the old movements, and so emotion and action appear simultaneously. In these cases, of course, the bodily changes are not caused by the particular emotion which they accompany. On the other hand, these reflexes are not the only 'expressions' of emotion. The original expression of anger, for instance, was doubtless a blow. The clenching of the fist points to restraining influences. But, though we do not now use crude physical measures exclusively, civilization does not leave us altogether helpless. The covert sneer, the insulting stare, the cutting remark are at our service. These and similar purposive actions require to be accounted for. The natural thing is to regard them as the effects of the emotion. There is an intelligible relation between them and the psychical state. An individual possessed with hate of some person will tell you that 'he feels as if he could'—do him all sort of injury. It is reasonable to suppose that this feeling conditions the action of retaliation. When it is asserted that this peculiar feeling towards the person only arrives after the action has actually taken place, we are at liberty to entertain a doubt in the

matter. When we find that no explanation whatever is given of the appearance of the action, our doubt will scarcely be diminished.

The organic changes, then, which are directly teleological, must be regarded as falling into two classes, namely, reflexes and voluntary movements. As Professor Baldwin shows, the effect-theory does not and cannot afford any explanation of the origin of the former in the first instance. The latter can scarcely be said to be taken into account at all, for the view is not worked out in detail. Cases like fear are used as instances where the action is practically the same under all circumstances, so that the reflex and the voluntary movements coincide.[1]

The internal organic changes remain, and Professor Dewey really faces the question here by attempting to show how they can be accounted for without reference to an antecedent emotion. They result from "the effort of the organism to adjust its formed habits or coördinations of the past to present necessities as made known in perception or idea. The emotion is psychologically the adjustment or tension of habit and ideal, and the organic changes are the literal working out in concrete terms of the struggle of adjustment" (Psy. Rev., II, 1, p. 30). The habitual reaction does not harmonize with the mode of action which the present circumstances seem to demand. Hence there is a conflict, and, as a result of this, disturbances of visceral and associated organs, "which is just what we might expect when there is a great stirring up of energy preparatory to activity, but no defined channel of discharge" (Psy. Rev., I, 6, 565). It is not easy to understand why the habitual reaction should assert itself so strongly in circumstances where it is obviously so much out of place, and insist on being 'coördinated' with the new mode of behavior. Nor is it obvious how emotion can be the 'tension of habit and ideal,' prior to action, when it is first constituted after the action has taken place (II. 1. pp. 18, 22). Leaving these points undiscussed, let us ascertain the results which follow from the explanation of the internal organic agitation here given. In the first place, the course taken by the deflected energy would seem to be mechanically determined as Professor James suggests (*Principles of Psychology*, II., p. 482). It will be dependent on the individual organism and its state at the moment. Professor Dewey is quick to see the effect of this. "If the bodily attitude is wholly accidental, then the emotion itself is brute and insignificant, upon a theory which

[1] In this connection one might safely venture the assertion that the theory under discussion would not seem so convincing if it were applied all round, instead of being stated generally, and merely illustrated by one or two favorable instances.

holds that emotion is the 'feel' of such an attitude" (I, 6, p. 563). He finds it 'more or less intolerable' that any idiopathic effects should be 'purely mechanical outpourings through the easiest available channel,' and maintains that the easiest path is determined by habits which, upon the whole, were evolved as useful (I, 6, p. 563). It would be very difficult, however, to prove that the bodily excitement could ever have been useful; and until such proof is offered, the 'intolerable' supposition of Professor James must be regarded as the more probable.

In the second place, under whatever circumstances the energy is aroused, the same amount spreading through the same organism at the same time will have the same effects. There is simply so much energy which is under a mechanical necessity to find an outlet. That the special occasion has no influence in determining the actual channels of discharge is rendered more obvious when we remember that the whole process is necessary just because action, appropriate to the particular case, has been inhibited. In every emotional state of the same intensity, therefore, the physical agitation will be practically the same; and this is a result more than serious to those who assert that the bodily changes cause the emotion. When the alleged causes are so much alike, why should the psychical effects be so widely different?

I do not criticise Professor Dewey for attempting to account for the internal organic disturbance without reference to emotion. It seems to me that he has shown that the phenomenon in question cannot be regarded as an effect of the emotion; and, further, that he has indicated the right principle[1] by which its origin is to be understood. My point is that the consequences have been shown to be disastrous to the theory he has adopted. For instance, one of the great difficulties which has to be met is that the bodily changes do not vary sufficiently in the case of different emotions, while they vary too much in different instances of the same emotion. The objection on this score was formerly made on the ground of empirical observation. Now, we can not only urge that the fact is so, but give a reason why, in the nature of things, it should be as it is. The escape of deflected energy is the cause of the internal effects, and these form the greater part of the whole mass of bodily change. Since the process is under mechanical law, the effects vary with the state of the organism; and this accounts for the fact that the same

[1] The principle must be worked out more fully, however, and freed from irrelevant additions. In all probability, too, it will be found necessary to supplement it to some extent by others.

emotion may at different times have different physical accompaniments. Further, under any outward circumstances a given amount of energy will always produce the same results if the inward physical conditions remain unaltered; and this explains why, in all states of equal intensity, the organic changes are substantially the same.

On the other hand, it seems possible for opponents of the effect-theory to state their views, so as to include and harmonize all the facts. You cannot proceed on the assumption that emotion must be either the cause or the effect of the physical changes. In the first place, the term 'physical change' is wide and vague. It covers movements, purposively reflex, voluntary, and mechanically determined. In the second place, the antithesis is false, for there is a third possibility, namely, that the psychical and physical aspects are independent of each other and yet concomitant. It is possible to hold, therefore, that some of the bodily changes are effects of the emotion, that others are independent of it, and that the emotion is in turn independent of the latter. When danger threatens, for example, it is possible to imagine that the perception is followed at once by fear and an arousal of energy, while certain actions or tendencies to action are called forth by association. All these arise simultaneously. We have at once the emotion of fear, the excitation of energy, and the tendency to crouch or run away. In the case of the first emotion of the kind, the third effect would not be present as a direct consequence of the cognition. The psychical state is a feeling with reference to the impending event prompting us to action of a certain kind with regard to it. The energy excited renders it possible for this action to be carried out with speed and vigor. If flight is out of the question, the energy diffuses itself through the organism and produces the violent organic paroxysm of intense terror. If the danger can be avoided and the means are suggested, the energy is used up for the most part in carrying out the necessary movements. Still a certain quantity always discharges itself through the body, for more energy is aroused than is absolutely necessary, and in most instances an interval must elapse before means can be found and the action put in train. Even though the purposive action is not inhibited, therefore, a certain degree of physical agitation is always present. I have purposely taken one of the cases which is most favorable to the effect-theory. The organic perturbation is not nearly so marked when, as in hate for instance, immediate and decisive action is not so vitally essential. The emotion may be as strong, however, for the physical changes vary in strength and prominence, not with the psychical concomitant, but

with the practical demands of the situation. Further, it is only in an instance, as simple as the one I have chosen, that it can be maintained with any show of plausibility that reflex action accounts for all the movements involved. In more complicated cases more complicated actions are necessary, and it is not so easy to exclude from consideration the influence of the purely psychical aspect of the concrete emotional state.

I can only refer to Professor Baldwin's interesting paper in so far as it bears on the special point now under discussion. This writer argues that "though habit means subsiding consciousness, it is just those 'expressive' reactions which are most instinctive, that carry with them most of the vivid and disturbed consciousness we call emotion" (Psy. Rev., I, 6, 612). Hence he concludes that the consciousness in question cannot arise until the instinctive reactions are reported back from the periphery. This view is open to criticism on many points, but I merely wish to point out that, thanks to Professor Dewey, it can be attacked on a question of fact. The argument depends on the assumption that all 'expressive reactions' are instinctive. Some are undoubtedly, but the greater number of them are caused by the deflection of energy or the spreading of excess energy through the body. They are not instinctive at all, but simply 'mechanical outpouring through the easiest drainage channels.'

We find, therefore, that nothing has been adduced on the basis of which it can be denied that emotion has a function of some kind, and causes some of the bodily movements which enter into the complete emotional state. The voluntary actions which seem to follow naturally from the peculiar psychical attitude are either left out of account altogether, or confounded with the instinctive reactions, which are themselves simply taken for granted. The explanation of the internal organic disturbance is in principle sound. It involves consequences, however, which are fatal to the effect-theory, and incidentally it destroys the special argument on the strength of which Professor Baldwin is induced to give a qualified assent to Professor James' main contention. On the other hand, opponents can accept it gratefully, and take account of it in framing their own theories.

David Irons.

Cornell University.

A REPLY.

If it is not contrary to editorial rules, I hope I may be allowed to protest against the unfair and dogmatic verdict which Professor G. S. Fullerton passes on the concluding section of my Aristotelian Address, 'The Conception of Infinity,' reviewed by him in the January number of this journal (Vol. II, Part I, p. 99), inasmuch as it is founded on a confusion of ideas which he attributes to me, though it is really due only to himself. The first part of his article is appreciative and generous, and for that I thank him. But when he comes to the sixth and last section of my address, which he calls my conclusion, he says that I reach it only by 'blowing both hot and cold in my premises,' which, supported as it is by a page of very plausible (though of course unintentional) misstatements, is an intolerable charge.

In three several cases on that page (99) he confuses distinctions which I clearly make and adhere to. He confuses :

1st—Between my 'material world' and my 'universe.'

2nd—Between my 'material element in consciousness' and my 'matter' or 'material world.'

3rd—Between my 'perceptually known' and my 'positively known.'

Now, in my argument, time and space are never perceived or represented in thought without some kind of 'material element in consciousness,' and in this sense the 'material element' shares their infinity ; the 'formal' and 'material' elements together being indispensable constituents of anything thought of as existing (even when it is a void that is thought of), and therefore of the infinite universe.

An infinite void when thought of as existing, either in time or space, is a void only in the sense of being empty of physical matter, and I have given some reasons in my address for thinking that the world of physical matter is finite, being bounded by a void in this sense, both in time and space.

When we know any specific content (under which term physical matter is included) existing in time, or in time and space together, that is what I call having a 'positive knowledge' of it. But though we have no such positive knowledge of anything beyond the limits of the world of physical matter, we may yet have a 'perceptual knowledge' of time and space beyond them, namely, a representation of them as filled with some 'material element in consciousness;' this element being an indispensable condition of them as existing, although it is not specifically known to us.

The inseparability of 'formal' from the 'material' element in consciousness compels us to think of the universe as infinite, so soon as we recognize that one of these elements is infinite, and cannot be thought of as being otherwise; unless we assume dogmatically that only *that* really exists which man has specific sense-faculties to perceive. It is this dogmatic assumption which really 'shuts us up to the world of matter.'

This argument of mine may or may not commend itself as conclusive; but at least it cannot be charged with attaching varying and inconsistent meanings to the premises employed, which is the charge made in the words I have quoted from Professor Fullerton, and made in the most peremptory fashion.[1]

LONDON. SHADWORTH H. HODGSON.

A NOTICE.

Dr. Herbert Nichols, formerly assistant of the Harvard Psychological Laboratory, publishes in the last number of the *Philosophical Review* a most violent attack on that laboratory under the title 'The Motor Power of Ideas.' His scientific motives, to which I must confine myself here, are based on such an absolute misunderstanding of our paper and his discussion is so full of misstatements that I consider it useless and hopeless to attempt to correct his mistakes and discuss his arguments. But there is one point which troubles me. The paper which he refers to is published by Mr. Campbell and myself. Those who read the kind words with which Dr. Nichols praised my work a short time ago may not believe that he can change his mind so suddenly, and some may suppose perhaps that the attack is directed especially against Mr. Campbell. Under these circumstances I take it as my duty to free my young friend, Mr. Campbell, publicly from all responsibility. Mr. Campbell carried out the experiments most carefully, but the whole plan of work was laid out by me, and I myself wrote every line of the article. It is probably superfluous to add that I should have written every line just as it now stands even if I had known Dr. Nichols' so-called criticism beforehand. HUGO MÜNSTERBERG.

HARVARD UNIVERSITY.

[1] [A farther comment from Prof. Fullerton on Mr. Hodgson's positions will appear in the next number.—Eds.]

PSYCHOLOGICAL LITERATURE.

DEGENERATION AND GENIUS.

Dégénerés et Desequibrés. J. Dallemagne. Brussels, Lamertin; Paris, Alcan; 1895. Large 8vo., pp. 658.

Dr. Dallemagne writes a very fluent colloquial style, well suited to the lecture-room, for which his book was originally written, and for this reason his pages, though immense, can be read quickly. The work is so full of details that our notice can hardly do more than recommend it to any reader who may be in search of a *vue d'ensemble* of the psychopathic constitution, and who at the same time has a good stomach for the great richness of the subject, and for the parenthetical sallys and sidepockets of reflection and description into which our author will drag him remorselessly. M. Dallemagne's reading has been enormous, and, so far as appears, exact; and it has in no way stifled his independence of judgment. His conception of the sphere of 'loss of balance' in the mental system is wider than that of Magnan, embracing Neurasthenia, Hysteria, and Epilepsy, of all which conditions the accounts he gives are both full of matter and up to date. Epilepsy, indeed, is the first subject treated after Imbecility, which latter follows six preliminary chapters on the psychological mechanism and on the notion and significance of the 'degenerative' type. The author allows that the conception of degeneration is still in process of evolution towards exactitude. It must indeed be admitted that causal and symptomatic notions mingle in it; heredity is an added element which is neither cause nor symptom; and a placid myxœdematous *crétin* has superficially nothing in common with a one-idea'd fanatic or other member of Magnan's class of *degenérés supérieurs*, except that both are 'queer.' M. Dallemagne has wrestled copiously with the problem, and perhaps better than anyone has emphasized the notion of *balance* as the test of mental health. Dissociation of the mental system, impulses so strong that they bear all inner opposition down, aversions and appetites at variance with the subject's beliefs, abrupt explosions interruptive of the consistency of his life, such are the traits of whole groups of psychopathic persons. M. Dallemagne even tentatively suggests a theory for such *disequilibration.* I find it very obscure,

but it suggests to my mind something like this: The thalami and corpora striata are 'subconscious' centres for habits, cravings and impulses that are not so 'saturated' with experience as to have become fatally automatic, like those in the cord. They normally act in coöperation with the fully conscious cortex and its associations of ideas. But they may become the seat of irritable weakness, and the associative cortical processes may be pathologically blocked or twisted from what is normal. In such cases the *poussée* from below is either excessive or ill-timed, and it may also fall on wrong ideas and the normally controlling ones be thrown out of gear. All sorts of obsessions, phobias, depravities of appetite, morbid impulses characteristic of the discordant self, which we find in the so-called degenerates, may be thus explained. In the 'inferior' class the cortex is most at fault; in the 'superiors' it is the basal ganglia.

Entartung und Genie, Neue Studien. Cesare Lombroso, gesammelt und unter Mitwirkung des Verfassers deutsch herausgegeben von Hans Kurella. Leipzig, Wigand, 1894. 12°, pp. 308.

A collection of essays and fragments, not published as a volume in Italian. The author first replies to some objections to his theory that genius is a degenerative neurosis allied to epilepsy and moral insanity. Dante, Michelangelo, and Guido had been thrown at him as examples of men of genius who were normal. He proves minutely their strongly eccentric and neurotic constitution. Dante in particular must have been frankly epileptic, for no less than eleven times in the divine comedy he speaks of himself as swooning or falling unconscious. That the weakness of genius cannot be due to secondary strains and fatigues incidental to the ardent sort of life which the possession of genius imposes, is proved by the fact that out of 313 symptoms of fatigue which Lombroso has counted, only six are commonly found among geniuses. Genius and sex is discussed in a chapter, full of anecdotes, on the conditions productive of genius. In Chapter III. is shown the frequency of degenerative anomalies in geniuses. For example, they vary from their national type, as is proved by portraits. Longfellow, Bellamy, Tennyson, Coleridge look like men of Latin race. Darwin and Bryant, Coleridge and Burns, George Eliot and Bulwer form mutually resembling pairs. "The cause of these resemblances is to be sought in the degeneration common to them all." Prof. Lombroso has compared the field of view of twelve geniuses of his acquaintance with that of eight unusually gifted young men who were not geniuses, and has found a shrinkage of the inner upper quadrant in nine of

the geniuses—in none of them was the field symmetrical. The non-geniuses were much more normal; so that an abnormal field of view seems to characterize genius. On the other hand, genius would seem to have, if anything, a slower reaction-time than usual. Amongst the *bizarreries* of genius, playing with orthography is mentioned, and a dog-latin letter of Swift to Stella is quoted. "One is tempted," Lombroso remarks, "to find in this tendency to fabricate a jargon, a trait connecting genius with criminality." The most valuable part of the book is constituted by biographical details concerning certain 'borderland' cases, calculating geniuses, thought readers, artists, and political and religious 'mattoids.' The author's curiosity and information, frankness, good-humor and vivacity are beyond praise, but his incapacity for accurate reasoning is apparently incurable; and this book, were it not for the biographic material which it contains, could only be regarded as one of the oddities of scientific literature.

Degeneration. MAX NORDAU. Translated from the second edition of the German work. New York, Appleton, 1895. 8vo., pp. 560.

A pathological book on a pathological subject. If one were to apply Herr Nordau's method to the description of his own person, one could hardly help writing him down as a degenerate of the worst sort. He is a 'graphomaniac'; a misanthrope and a 'misoneist'; a 'coprolalic' ('idiot,' 'imbecile' are his mildest terms of endearment); an 'erotomaniac' of the prudish sort, haunted by horror of other people's sexuality; an *obsédé*, pursued without respite by images of odious works of art; a 'megalomaniac' of the arrogant and insulting type; and, finally, a victim of insane delusions about a conspiracy of hysterics and degenerates menacing the moral world with destruction unless the sound-minded speedily arm and organize in its defence. Add to this equipment the earnestness of the gloomily insane, and their complete inability to see a joke (pages of heavy invective against Oscar Wilde's epigrams!) and one gets a not altogether consoling diagnosis of Herr Nordau's case. On the other side, it must be admitted that he is really learned, not only in contemporary German, French and English *belles lettres*, but in the literature of neurological medicine as well, and that many of the objects by whose odiousness his imagination is afflicted, Parisian 'pornographic' novels, for example, are loathsome indeed. When, however, hardly a contemporary name, however great, escapes his abuse, and the course over which he runs-a-muck lies through Wagner, Tolstoi, Ruskin, Burne-Jones, Rossetti, Zola, Ibsen, and Niet-

sche, as well as through Baudelaire and his descendants, it must be admitted that his volumes are little more than a pathological 'document' on an enormous scale, and an exhibition in minute detail of an individual's temperamental restrictions in the way of enjoying art.

The only chapters that concern this REVIEW are those entitled 'the psychology of mysticism' and 'the psychology of egotism' respectively. Mysticism and egotism are the great mental stigmata of hereditary degeneration. Mysticism is, in brief, the tendency to see in everything more than appears on the surface, and to suppose mysterious significance in the plainest facts. Its condition, our author says, is an inability of voluntary attention to confine the flow of association. In the exhausted and aimless brain of a degenerate, beyond the clear and immediate associates of an idea, there surges up circle beyond circle of remote associates, pale and vague reverberations of distant ideas, which make all perceptions spectral and all judgments uncertain. This is much like saying that in a healthy mind thoughts should have no atmosphere, no overtones, no fringes —an opinion to which few will subscribe.—Herr Nordau's explanation of the egotism of degenerates is based on the observations of Sollier and others upon imbeciles, and of Lombroso upon criminals, showing obtuse sensibility of the skin and other perceptive organs. Whilst the outer world thus comes to them and their congeners imperfectly, the inner world, on the contrary, fills them with its clamorous impulses and obsessions; their enfeebled will cannot hold the balance, the line (arbitrary at best) between the me and the not-me is shifted, and the me fills the field of attention. This theory, also, though it has its ingenuity, is one which psychologists will hardly find completely satisfactory.

The translation, so far as I have examined it, reads fairly well. But the publishers have made 560 very vast and ugly pages out of the 1000 odd convenient pages of the two-volume original.

Genie und Entartung, eine psychologische Studie. WILLIAM HIRSCH. Leipzig; Coblentz, 1894. 8°, pp. 340.

It really reanimates one, after so much farce-comedy writing on the subject of genius, to come upon a book based on psychological analysis, logic, and common-sense. It would be well if all the admirers of Lombroso, Nisbet, and Nordau could be compelled to read Dr. Hirsch's admirable study, of which every page is interesting and acute. I can only quote general principles from it, leaving out details. In the first place, the author remarks, 'genius' is a sociological, not a psychological concept. The class of persons popu-

larly recognized as geniuses—poets, musical composers, musical executants, actors, painters, men of science, statesmen, soldiers, and devotees—seem at first sight to have nothing in common except rarity and originality. But originality itself is not a psychological conception. An Englishman appears in France as 'an original'; and so does a Yankee in England. The oddity of lunatics, due to recklessness and non-inhibition, has nothing inwardly in common with the fertility in novel ideas that characterizes geniuses. "In different fields [as the author shows by detailed discussion] we see that the most diverse psychological elements constitute the man's genius, and that qualities which in one case make its essence, are in others actually incompatible with its activity. There are, therefore, no definite psychological qualities common to all geniuses. One would seek in vain identical features in Bismarck and Paganini, in Mozart and Napoleon." Even within one type there are tremendous differences, Goethe and Beethoven, for instance, having been men of mood and inspiration; Schiller and Mozart, men of continuous activity independent of mood, working with will and critical reflection. The mental *elements* are identical in the sane and the insane, the difference between these classes of men being one of proportion and mixture. With strong obsessions one needs a strong will to keep sound, just as with a large body one needs large legs to keep active. The excessive sensibility of a Goethe would have made a psychopath of him, but for his extraordinary intellect and self-control—with these additions it simply made him the mightier pattern of mankind. The logic which, because it finds hallucinations in crazy people, treats them thenceforward as an insane symptom, even where no other insane symptom is present, begs the question. Their existence in sane men should on the contrary be held to prove that they are not necessarily a morbid symptom. The 'simple enumeration' of geniuses with psychopathic traits, to prove the essential psychopathy of all genius [apart from the fact that by the same logic one could prove that being born on a Sunday, or having brown eyes, was genius's essential condition], is carried out with no pretence to exactitude. Rightly used, the statistical argument ought to ascertain, first, the percentage of the mentally unsound in a given population at a given moment, then the total number of geniuses, and finally the number of mentally diseased geniuses, in the same population at the same moment. If the percentage of disease were higher in the geniuses than in the population, the neurosis theory of genius would receive presumptive support. It is needless to say that such statistics are unattainable, nor are they attempted

by any of the advocates of the neurosis theory.[1] The close of Dr. Hirsch's book has much to say *in rê* Nordau *versus* Wagner and Ibsen. This critical matter calls for no reproduction here.

If the reviewer might now say a word of the result left on his own mind by reading the genius-controversy, it would run something like this: Moreau, Lombroso & Co. have done excellent service in destroying the classic view of genius as something superhuman and flawless. By their ferreting and prying and general devil's advocacy, they have helped us to an acquaintance with human nature *in concreto*, which from every point of view is superior to our old-fashioned academic notions. Lombroso in particular has put us in his debt by his studies of individual fanatics and 'mattoids.' But there the service stops, for (except in Nordau's case) these authors are incapable of logical or psychological analysis; and the only conclusion that their facts make more clear than ever—the conclusion, namely, that there are no incompatibles in human nature, and that any random combination of mental elements that can be conceived may also be realized in some individual—is one that they do not draw. If we are to make of genius a psychological conception at all, it must be a property of *intellect* rather than of will or feeling. Narrowed in this way, Prof. Bain's description of it, as an unusual tendency to associate by similarity (a description with which none of our authors seem acquainted), will stand firm. But it is one thing to have this intellectual condition of genius and another to become effective in

[1] The only attempt to use statistics methodically by advocates of the theory is in Nisbet's comparison of the causes of death in genius and in the population at large (see "The Insanity of Genius," 1893, p. 315). This author says: "I have dealt specifically with some 250 men of genius. Selected upon no other ground than their eminence, in the first instance, the *total number* of these are found to be neuropathic, suffering from or dying of some description of nerve disorder." Mr. Nisbet then enumerates amongst the "chief constitutional or nerve diseases": Phthisis, pneumonia, apoplexy, heart disease, scrofula, rheumatism, syncope, diabetes, gout and stone, and then tells us that the death-rate of the 'nerve diseases,' thus Pickwickianly understood, to the entire death-rate of Great Britain in 1888, was as 1 to $3\frac{1}{2}$, whilst "among the men of genius enumerated it is at least three times greater." Ignoring the ridiculousness of Mr. N.'s classification of nerve diseases, and assuming that all the 250 geniuses really did die of them, not the total population, but some part of the population whose pursuits are intellectual, was the proper term of comparison. If Mr. Nisbet had looked up the personal and family history of 250 occupants of offices in the city, or of 250 active business men, and found less gout and apoplexy than in his geniuses, it would have been a really interesting fact, for these are diseases of sedentary life as such, and geniuses are on the whole of the sedentary class. Similarly before the 'family history' of geniuses can be held to prove the neurosis theory, a comparison must be made with the families of an equal number of business men, 'selected upon no other ground' say than their wealth. No such comparison has ever been attempted.

history *as* a genius, and to figure in biographical dictionaries. We all know intellects of first-rate original quality whose names are written in water because of the inferiority of the other elements of their nature, their lack of remote ideals and unifying aims, of passion and of staying power. On the other hand we know moderate intellects who become effective and even famous in the world's work because of their force of character, their passionate interests and doggedness of will. To *do* anything with one's genius requires passion; to do much requires doggedness. Hence it comes that the intense sensibility of the psychopathic temperament, when it *adds* itself to a first-rate intellect, greatly increases the chances that the latter will bear effective fruits. To be liable to obsession by ideas, not to be able to rest till they are 'worked off,' ought then to be, as they indeed are, traits of character often found amongst the men whose names figure as those of geniuses in the cyclopedias. But these traits have no essential connection with the sort of intellect that makes the men geniuses. We may find them combined with any sort of intellect, as we find first-rate intellect combined with any sort of character. The names of Emerson, Longfellow, Lowell, Whittier, and Holmes would probably be those first written by any one who should be asked for a list of the geniuses of the community in which I write. Although belonging to the class of poets (the species of genius most akin to psychopathy by the sensibility it demands), these men were all distinguished for balance of character and common sense. So Schiller, so Browning, so George Sand. In poets like Shelley, Poe, de Musset, on the other hand, we have the intellectual and passionate gifts without the powers of inhibition. In the sphere of action we have a similar diversity of mixture: we find the all-round men like Washington, Cavour, and Gladstone; the great intellects and wills with no hearts, like Frederic the Great; the intense hearts and wills with narrow intellects, like Garibaldi and John Brown; the stubborn wills with mediocre hearts and intellects, like George III. or Philip II.; and, finally, the real cranks and half-insane fanatics, often with much of the equipment of effective genius except a normal set of 'ideas.' It all depends on the mixture; only as the elements vary independently, the chances that a freak of nature in the line of human greatness will be as exceptionally strong in all three elements of character as he is in any one of them, is small. Hence some lop-sidedness in almost all distinguished personages, hence the rarity of the Dantes, St. Bernards, and Goethes among the children of men.

One more word: there is a strong tendency among these pathological writers to represent the line of mental health as a very narrow crack, which one must tread with bated breath, between foul fiends on the one side and gulfs of despond on the other. Now, health is a term of subjective appreciation, not of objective description, to borrow a nomenclature from Prof. Royce: it is a teleological term. There is no purely objective standard of sound health. Any peculiarity that is of use to a man is a point of soundness in him, and what makes a man sound for one function may make him unsound for another. Moreover, we are all instruments for social use; and if sensibilities, obsessions and other psychopathic peculiarities can so combine with the rest of our constitution as to make us the more useful to our kind, why then we should not call them in that context points of unhealthiness, but rather the reverse.

The trouble is that such writers as Nordau use the descriptive names of symptoms merely as an artifice for giving objective authority to their personal dislikes. The medical terms become mere 'appreciative' clubs to knock men down with. Call a man a 'cad' and you've settled his social status. Call him a 'degenerate,' and you've grouped him with the most loathsome specimens of the race, in spite of the fact that he may be one of its most precious members. The only sort of being, in fact, who can remain as the typical normal man, after all the individuals with degenerative symptoms have been rejected, must be a perfect nullity. He must, it is true, be able to perform the necessities of nature and adapt himself to his environment so as to come in when it rains; but being free from all the excesses and superfluities that make Man's life interesting, without love, poetry, art, religion, or any other ideal than pride in his non-neurotic constitution, he is the human counterpart of that 'temperance' hotel of which the traveler's handbook said: "It possesses no other quality to recommend it." We all remember the sort of school-boy who used to ask us six times a day to feel of his biceps. The sort of man who pounds his mental chest and says to us: "See, there isn't a morbid fibre in my composition!" is like unto him. Few more profitless members of the race can be found. The real lesson of the genius-books is that we should welcome sensibilities, impulses and obsessions if we have them, so long as by their means the field of our experience grows deeper and we contribute the better to the race's stores; that we should broaden our notion of health instead of narrowing it; that we should regard no single element of weakness as fatal—in short that we should *not be afraid of life.*

W. J.

GENERAL.

Thèorie de l'ondulation universelle; essais sur l'évolution. B. CONTA. Paris, Alcan, 1895. Pp. 216.

Perhaps the thing of greatest interest in this volume of metaphysics is the biography of the author. Conta, born in Maldavin, of illiterate parents, by turns conjuror and student, constantly changing his profession to make his living, but always pursued by the same sensations of hunger and phthisis, ended by becoming professor of law, and died Minister of Education of his country. Such a life proves that he was a man of unusual energy. As for his theories of universal undulation—which are not on the whole very different from the theories of evolution—they have very little interest for us, so schematic are they and distant from facts! Toward the end of his book there are some curious details on what Conta calls laws of universal assimilation. I say curious because the author, hastened and hindered by the malady which finally carried him off, had not time to develop his thought, and he has thrown together simple notes, and these notes seem to me to explain his method very clearly. It follows from the first idea that all bodies tend to assimilate others by each communicating its own peculiar external and internal movement. This applies in the first place to purely physical phenomena: so a body that presses (this is his own expression, p. 208) communicates the direction of its own displacement to that of the body that is pressed, etc. Then come the physiological phenomena: the particles which go to make bone are transformed into bony matter. Then he passes to contagion of diseases, without being aware of the abruptness of the transition: the different degrees of unhealthiness are communicated from individual to individual. Finally, the author cites, as being part of the same series, psychical influences. We experience the emotions of those with whom we happen to be; we laugh with those that laugh, and weep with those who weep, etc. All phenomena attributed to imitation belong to assimilation: "all phenomena produced in the nervous system of the person who influences is communicated with more or less force to the nervous system of the person influenced" (p. 211). I think that this series of arguments gives us enough light on the value of this work. A. B.

SORBONNE, PARIS.

La Vie et la Pensée: essai de concéption experimentale. G. PIOGER. Paris, Alcan, 1894. Pp. 260.

In this book M. Pioger aims to trace the development of organic life into conscious and mental life, and to this end he gathers and

coördinates facts from the experimental sciences. Hence he adds to his title the phrase 'experimental conception' or the systematization of our *real* knowledge. Such knowledge is confined to our thought of the relations which we find among things, for we cannot penetrate into the *substance* of things. Yet it is *true and objective* knowledge in spite of Berkeley (?); and this knowledge enables us to conceive the world experimentally—that is, to systematize the relations which we perceive and to embrace them as a whole by thought.

Our thoughts are produced by the special orientation of our psychic sensibility, which suffers the influences of the environment of which it is a part, and of which the most intimate parts also go to compose it. So thought is prepared for specific organic and vital functions. It appears at the moment that what M. Pioger calls *mentality* arises from the relation of certain vital elements. Mentality is that which *personifies* our intellectual aptitudes and gives us our mental constitution. This constitution, therefore, takes root in our organic constitution, and that in turn in our inorganic constitution—a result from the solidarity of inorganic elements. From mentality and its phenomena we reach the concept of consciousness, which is a mid-term between life and thought.

Consciousness, therefore, plunges its roots into the depths of our lower life and pushes its branches up into the intellectual and social. M. Pioger gives a table showing the various ramifications.

The individual consciousness is, then, only a preparation for social life and consciousness, whose phenomena arise from the reciprocal action and articulation of social elements, just as in turn the phenomena of mentality and intelligence result from the articulation in the sphere of the individual's psychic sensibility. To the first part, which gives the analysis of the elements constitutive of thought and life, and shows the lower regions in which they lie hidden, M. Pioger adds a second part devoted to the synthesis of elements. He shows the solidarity which they come to present in the individual (in thought), in the race (by heredity), and in society.

To sum up, the book is an attempt to throw together some of the data of experimental science from a point of view similar to Spencer's, but narrower. But the data are arbitrarily chosen, and the results are in many cases open to dispute. It is not based on original or new research.

J. PHILIPPE.
SORBONNE, PARIS.

Peregrinazioni Psicologiche. T. VIGNOLI. Milan, Hoepli, 1895. Pp. 404.

This is a collection of notes and essays published on various occasions between 1882 and 1894. The title of the book is justified

not only by the variety of subjects it handles, but also by the lack of any studied arrangement, neither the order of time nor that of topics being strictly observed. Each article, however, is marked by lucidity of exposition and by a wealth of details which, though familiar for the most part to students of psychology, are made to do good service for the critical and constructive purposes of the author.

The volume would not have suffered seriously by the omission of the discourse on 'The Paleontology of the Spirit,' as this is merely a bit of sarcasm expended on unscientific notions—'fossils of the mind.' Of the remaining articles, that which possesses the most actual interest is on 'colored audition.' To understand this phenomenon we must recall the facts of brain-growth. The protoplasm was the seat of general sensation. As the various tissues, organs and centres were differentiated, the original aptitude to receive all sorts of impressions remained in a latent form. Its revival explains those 'organic metaphors' whereby a single impression gives rise to different sensations. This explanation, however, is put forward simply as an hypothesis, with a prudent 'perhaps' here and there.

'Paramnesia' the author handles with more assurance. Such peculiarities of memory, far from being abnormal, are accounted for by three causes: the reproduction, by association, of ideas, images and feelings; the rapidity of thought; and the automatic constructive power of mind and imagination. Because the mind, when it perceives an analogy between a present object and one that was previously perceived, fails to distinguish one from the other, it transfers the actual image to an indefinite past. The comparative judgment is inhibited partly by the rapidity of thought and partly by the unconscious character of one of the factors.

To 'certain unconscious intervals in a coördinated series of psychic acts,' a lengthy study is devoted. That such intervals are possible is shown from numerous facts of memory, dream-picturing and the ordinary activity of the waking state. They are filled in by cerebral functions, which, though they do not rise into consciousness, are capable, because of the laws of heredity, of linking one conscious state with another.

An inquiry into the 'genesis of our sense-perceptions' leads, by delicate analysis of the physical and physiological processes, to the vexed question—How does the brain-motion become sensation? The answer is facilitated by a comparison. Between the qualities which an element acquires in passing from one allotropic state to another, and its molecular structure, there is no relation that we can discern. Nor is there, so far as we can perceive, any relation be-

tween the physiological process and the sensation. They are two states. The physical state and the psychical state are the eternal and fundamental forms of the universe.

Five of the articles contained in the volume, though treating of different subjects, present a certain similarity. Thus the growth of 'the moral sense' is explained according to the laws of evolution, and especially of heredity. Man's vicious proclivities are the effects of atavism, of reversion to a pre-human condition from which man emerged by an act of reflection. To make the results of this act prevail over the atavistic tendency, is the secret of social progress. Again, 'attention' being widened out till it means 'response to a stimulus,' it is found to be essentially the same throughout the animal series. Only in man there is a power of introspection whereby he can attend to the very act of attention; and this it is that distinguishes him from the lower animals. The same line of demarcation is drawn in the article on the 'origin of articulate speech.' Man has, in common with other animals, a 'physiological language'; but this is fundamentally different from speech. The latter is not a copy but a symbol of the internal process. In man thought precedes speech; so that from the articulation of the one by reflection there results the articulation of the other—its outward expression.

The importance of the 'sensory image for the development and exercise of intelligence,' arises from the very vagueness of the image that would seem to be an imperfection. Our perceptions are generic, *i. e.*, they give us but a small part of the details which the object really contains. This is required in all animals by the necessities of existence; since life would be impossible if a minute examination of each object had to be made. In man, moreover, the generic character of perception aids intelligence and gives rise to science, by serving as a means of classification and ulterior abstraction. The act of reflection being proper to man, extreme caution must be used in interpreting those actions of animals which seem to be on a par with those of human intelligence. On this principle, and on his personal observations, Vignoli criticizes with considerable keenness the accounts given by Lubbock and others of the dog's reading and counting abilities, and shows, how in these respects, the animal is inferior to the child and the savage.

His 'notes on a psychology of sex' are divided into two parts. In the first he outlines the intellectual, moral and industrial traits by which the sexes differ, and which depend upon organic and functional differences. In the second he insists that man, from the

beginning, must have been a social being; otherwise, articulate speech would never have been formed.

WASHINGTON. E. A. PACE.

Philosophy of Mind: An Essay in the Metaphysics of Psychology. G. T. LADD. New York, Charles Scribner's Sons, 1895. Pp. XIV + 414.

It is a little more than a year since Professor Ladd ended the preface to his *Psychology, Descriptive and Explanatory*, with a promise to deal in a later work with the philosophical problems which empirical psychology suggests. This promise is fulfilled in the volume before us. The field covered does not include all the questions to which psychology gives rise—it would need a system rather than an 'essay' for that—but the author selects for treatment a number of topics which are intimately connected with current discussions, and which possess an abiding interest. The standpoint from which these are considered is that of the 'empirical science of mental phenomena.' As Professor Ladd says (p. 82):

"Indeed, this essay in the philosophy of mind is deliberately based upon previous long-continued researches into the facts and laws of a scientific psychology. * * * And it is the author's controlling wish that the validity of the following speculative conclusions should constantly be brought face to face with the conclusions of the empirical science of mind."

Here, whether it agrees with Professor Ladd's views or not, the psychological world will be a unit in according him the praise which is due the patient endeavor to base metaphysics on the only secure foundation. No one among us has more earnestly studied or more carefully sifted the data and the outcome of scientific psychology than Professor Ladd; no one, therefore, is better entitled to claim for his results the consideration which of right belongs to thinking of such a character.

Further, as psychologists are already acquainted with the author's empirical conclusions, so they will find his metaphysics familiar almost at a glance. In general it is distinctly Lotzean in tone; while Professor Ladd's special opinions have been foreshadowed in his earlier works, including his *Introduction to Philosophy*. Both these points are evident from the metaphysical preludes with which many of the chapters of the present work begin, as well as in the conclusions reached. For instance, reality is thus defined (p. 120):

"Every real being is known as a self-active subject of states, standing in manifold relations to other beings, and maintaining its right

to be called real by acting and being acted upon,—only, however, in obedience to certain laws (or uniform modes of its behavior as such a being and no other)."

Again, concerning the consciousness of identity, it is argued (p. 151):

"Every x (every 'Thing' whatsoever), in order to be entitled still to be called x (or the 'self-same' thing) must in its changes run only through series such as can be indicated by $x, x^1, x^2, x^3, x^4 \ldots x^n$; or on occasion of its coming into other relations with different beings, the series may be that indicated by $x, x^\alpha, x^\beta, x^\gamma, x^\delta \ldots x^\omega$."

And it is concluded:

"The real identity of any thing consists in this, that its self-activity manifests itself, in all its different relations to other things, as conforming to an immanent idea." Similarly unity in anything whatever is held to imply 'the presence of some ideal in the very being of the thing' (p. 191), and self-consciousness, in its unitary development, to yield the best, if not the only conception of what a unit-being is; permanency in things and minds alike is deemed a matter of inferred rather than of direct knowledge, and the permanent being of mind is believed interrupted when consciousness lapses, except for the modicum of existence which consists in 'a certain abiding relation to all reality' or 'the world-ground' (p. 392).

The interest of psychologists, therefore, will centre about the way in which these two familiar elements of Professor Ladd's thinking are combined and the results to which his inquiry leads him. The book opens with two chapters on 'Psychology and the Philosophy of Mind.' The chief thesis here is the impossibility of divorcing psychology and philosophy altogether. This will be admitted by all—as to the latter end; for that psychology issues in the problems which philosophy discusses, is not susceptible of doubt. That psychology as a science actually does, and of necessity must, include metaphysical assumptions in its course, should be equally clear; although, no doubt, many will question the truth of the proposition. It is an easy task for Professor Ladd to show that the natural science, on the level of which our 'new psychology' delights to stand, is itself 'shot through' with metaphysical elements; and just as easy, though the work is rather more novel, to prove by examples—Höffding, James, and Flournoy are cited—that the professed rejectors of metaphysics are among the chief offenders against their own first principle. The nerve of the argument appears, however, in the conclusion that the only legitimate choice left for the

psychologist is between the uncritical dualism of common life and the adoption of 'some definitive metaphysical point of view' (p. 42) of his own selection, as has been done by Volkmann and Wundt. This may be the alternative in the present transitional condition of psychology. But surely history points toward a better ideal for the future, namely, the critical—though not always reflectively critical—determination and adoption by all of such principles as will best subserve the progress and the exactness of the science. It was in this way that the rising sciences of the modern era threw off the trammels which formed their heritage from Aristotle and the mediævalists; thus physics, to take a more special example, has in our own time been criticising some of its fundamental concepts, although to students of philosophy its advance may seem painfully hesitating and slow. So also psychology is still in the throes of its new birth. And when the happy time shall come for us to be fitted out with even as good a set of *working principles* as that which the physical sciences of the day enjoy, we shall be secure from the vagaries of the 'psychologies without a soul' on the one hand, and the necessity of constant re-discussion of our primary assumptions on the other. But on any view of the case, it must be regretted that Professor Ladd's polemic manner lags behind the material force of his arguments. The use of horrible examples is always a dangerous expedient in a technical treatise; and the psychological world will unite in deploring the characterization of Höffding's introduction of metaphysics into his psychology as a 'covert effort' (p. 22) and James's positivistic attitude as the 'position of materialism' (pp. 28, 39).

Chapters III–VI constitute the kernel of the volume. The chief positive outcome of the first of them, on 'The Concept of Mind,' is the emphasis laid on the element of self-activity in all self-consciousness. Chapter V, on the 'Consciousness of Identity and So-called Double Consciousness,' is the paper presented by the author at the last meeting of the Psychological Association; together with Chapter VI, on 'The Unity of Mind,' it advocates identity and unity as real predicates of the self, on the basis of the unity of the life of consciousness and in the sense of the definitions above cited. Chapter IV deals with a question central to the whole discussion, 'The Reality of Mind.' Noëtically, it is argued here, 'knowledge implicates reality,' and, metaphysically, all the marks of reality belong to the mind, known as a 'here-and-now-being' in self-consciousness and as a 'then-and-there-being' in memory, and inferred to be a permanent existence by reflective thought working on the data of

experience. Hence, also, it is concluded as a corollary, "The peculiar, the only intelligible and indubitable reality which belongs to Mind is its being for itself, by actual functioning of self-consciousness, of recognitive memory, and of thought" (p. 147). Yet it is with a sense of disappointment that the reader ends the chapter. The difficulty is partly one of method. In putting his most important thesis so early in his work the author has lost the advantage of the several arguments, positive and negative, which later on might have been combined into a proof of cumulative force. It is partly a difficulty with Professor Ladd's theory of knowledge, at least in so far as he has yet announced it. After diligent study of his various works, the present writer inclines to the belief that his first principle, 'knowledge implicates reality,' knowledge and reality are correlates, etc., might '*in some sort*' be acceptable to many of those not agnostics or phenomenists. But when this is used as a kind of universal major, with little or no systematic determination of subordinate criteria, especially when the psychology and the noëtics of self-consciousness are so intermingled that it is often impossible to decide on the basis of which of the two the argument is proceeding; the effect is not only confusion concerning the meaning of Professor Ladd's reasonings, but doubt in regard to their validity. But the difficulty arises partly, also, from an underestimation of the strength of opposing theories. The same failure to realize the importance of the reinforcements which have come to the cerebralists and materialists from the newer researches that marked Part III of the *Physiological Psychology*, reappears in the present treatise. And this, though the psychological world owes a debt of gratitude to Professor Ladd for the earnest defence of the reality and spirituality of mind which he has given alike in the earlier and in the later work.

Chapters VII–VIII, on 'Mind and Body,' are for the most part an elaboration of chapters XI, XXI and XXII of *Psychology, Descriptive and Explanatory*. But chapters IX, 'Materialism and Spiritualism,' and X, 'Monism and Dualism,' are among the most successful in the book. In the former vigorous blows are dealt the materialistic theory, without yielding to the claims of spiritualism in the monistic sense of the term. In the latter a still more forcible assault is made on both the scientific basis and the metaphysical deductions of the current psychological Spinozism. In Professor Ladd's own words (p. 344):

"In brief, then, the alleged scientific principle of psycho-physical parallelism is far from being the self-evident conclusion of modern psycho-physical research which it is so often and so rashly assumed

to be. Even the simplest relations between the phenomena of the lowest order of consciousness and the concomitant cerebral activities are far too fluctuating, complicated and changeable to be subsumed under this principle. Of parallelism in space we cannot speak appropriately in this connection. Of parallelism in time there is only an incomplete and broken analogy. And when one tries to think out clearly the conception of a complete qualitative parallelism, one finds the principle soon ending in inadequacy, and finally becoming unintelligible or absurd * * *."

Nor if the doctrine of psycho-physical parallelism, in the fullest meaning of the phrase, were proven, would monism necessarily follow. Rather the clearest inference would be to a moderate dualism, even though it is difficult to share the author's confidence that the 'double-aspect' theory is utterly meaningless.

The remaining discussions of the 'Origin and Permanence of Mind' and the 'Place of Man's Mind in Nature' point forward to the future development of Professor Ladd's views on ethics and the philosophy of religion. These will be the more eagerly awaited in view of the value of the present volume, which, in spite of defects, is one of the most notable contributions of recent years to the literature of the subject.

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Elements of Psychology (*Syllabus of Philosophy I*). J. H. Hyslop, Columbia College, New York, 1895. Pp. 130.

This syllabus is best described in Dr. Hyslop's own words: "As a time-saving instrument in my lectures on the subject, and as a guide to my students in their reading and study." It has also a personal interest in showing those who have been instructed by the author's work in other departments—logic and ethics—his general conception of the psychological area and its problems. Aside from these two uses it is hard to see what purpose it can serve. The analysis, while systematic and thorough as analysis, (excepting the chapter on emotion) does not allow one to see far enough into Dr. Hyslop's mind to warrant confidence as to one's insight into what his psychological position really is. This, of course, is a defect, if defect it be, not in execution but in original design ; for Professor Dewey has recently shown in his 'Study of Ethics' the possibility of a syllabus which shall contain both outline and suggestive doctrine. The analysis is indeed so thorough and comprehensive that here at least we believe that 'the part is not worth more than the whole,' and we hope that Dr. Hyslop may see fit some time to give his lec-

tures text-book form. The balance between the introspective and the 'extrospective' methods is, so far as can be judged, well preserved, and the author's breadth of view and psychological tolerance are well shown in his use of the observations of the 'psychic researchers.' How justifiable his particular use of these observations is, is another question (witness, for example, the remarks on the phenomena of retention), for the condensed analytic outline, because of its necessary meagerness, warns one off the field of interpretation and criticism. This is shown again in the remarks made on subliminal consciousness.

Particularly striking is Chapter X, on the 'Will or Conation.' Psychological students owe the author a debt of gratitude for this piece of analysis. It is a valuable supplement to the chapter on the 'Freedom of the Will' in the Ethics. These two chapters throw light on each other.

R. B. JOHNSON.

MIAMI UNIVERSITY.

Popular Scientific Lectures. E. MACH. Translated by Thomas J. McCormack. Chicago, The Open Court Publishing Co., 1895. Pp. 313. Price, $1.00.

Nearly all the lectures constituting this volume deal with the physiological or psychological side of physical questions; four treat of the methods and development of science, briefly outlining a theory of cognition.

Science, according to Prof. Mach, is essentially an *economy of thought.* Rooted in the most primitive psychical functions of life, this economy reaches its highest perfection in language and mathematics. Here, in the psychological origin and nature of our ideas, the elucidative power of the principle is obvious.

A natural law is a rule or body of directions for the mental reconstruction of facts; enabling us to anticipate and retrace in thought the steps of nature. To this end it embraces *only certain aspects* of the facts, such as are determinative. By means of these determinative elements and their formal constituents, we complete in thought *facts that are only partly given;* derive complete results from incomplete data. To reach the ungiven elements we should, on the primitive plan, have to resort to slow and tedious experience; that infinite pains we *save* ourselves by economical natural laws. This is all that science accomplishes. Its individual results we could reach in a sufficiently long time directly and without methods.

The mental reconstruction of facts we accomplish by *description,*[1]

[1] The view that 'explanation' is merely the description of unknown phenomena in terms of known phenomena was stated by Mach before either Clifford or Kirchhoff.

rendered necessary by communication, effected by comparison. Of description there are two kinds: direct and indirect. We describe a fact directly when we employ terms having an *abstract* import only, where our comparisons suggest only conceptual relations. For example, the definition of quantity of heat, being a definite numerical statement of a certain determinate relation between temperatures and masses, is a direct description, involving no adscititious notion, and having validity whether heat is a substance or a motion; and the same is true of energy. Now Black, in formulating the definition of quantity of heat, and Mayer and Joule, in stating the law of energy, viewed the facts under the notion of a *substance*; in so doing they resorted to *indirect description*—they employed a simile in which unessential and superfluous features were involved. Of such a character are *theories* in science, the wave-theory of light, heat as a motion, etc., useful in the preliminary steps of research, of unmistakable power as heuristic agents, but destined to be discarded when that final consummation of knowledge is reached—the *simplest and completest possible abstract expression of the facts.* Direct description is the goal of all research. Moreover, the method of physiological psychology is the same as that of physics.

Finally, is science, description, the ultimate unanalysable term in knowledge? Viewed in its higher collective relations, and qualified by the restrictions incident upon such a view, it is. We seek in philosophy an *integral* aspect of the universe; but our method is the *differential* method. It is in the latter that we must seek the foundations and conditions of our knowledge, not in the former. That economy which is embodied in our thoughts is conditioned upon the formal needs of the mind, but it is not necessarily to be found in nature.

T. J. McCormack.

SOCIAL PSYCHOLOGY.

(1.) *An Introduction to the Study of Society.* A. W. Small and G. E. Vincent. Am. Book Co., 1894.

(2.) *Les Transformations du droit.* G. Tarde. 2 ème ed. Alcan, 1894.

(3.) *L'Imitation et la logique sociale.* R. Berthelot, Revue de Metaphysique et de Morale. 1894. Pp. 93–97.

with whose names it is usually associated. The theory, natural enough in its origin, is not new in philosophy, although propounded independently by all the above-mentioned inquirers and rendered exact only by their definitions.

(4.) *Le Problème de la sociologie.* G. SIMMEL, idem. Pp. 497–504.
(5.) *Les Règles de la methode sociologique.* E. DURKHEIM, Revue Philosophique, May–August, 1894.

The province and method of sociology are at present a centre of discussion. As sociology in one of its aspects is social psychology, this involves discussion as to the possibility of a social as *vs.* an individual psychology, and, if this be admitted, as to the natural relations of the two.

1. The *province of sociology* is, according to Professor Small, threefold. Descriptive sociology is the science of the coördinated facts of society as it is; statical sociology is the science of social ideals, an approximate account of the society which ought to be; dynamical sociology studies the available resources for changing the actual into the ideal. Passing over the last two, descriptive sociology is more particularly "the organization of all the positive knowledge of man and of society furnished by biology, anthropology, psychology," etc., etc., and "attempts to combine the testimony of these special sciences into a revelation of the accidental and permanent factors in social combinations." The other writers are all inclined to seek a narrower, more specific field. M. Tarde (whose numerous works demand a special treatment) finds the essential characteristic of social phenomena to be 'imitation,' understood in the sense of "every reflection of one mind in others, of one will in others." Similar social phenomena (those of law are studied in particular) may be due either to 'invention,' in response to the demands of the environment, which refers us to a biological cause, or to imitation, a social cause. It is then this latter class of relations which forms the subject of a 'pure' sociology, as distinguished from biology and history. M. Berthelot, in an acute review of Tarde (Revue de Met., 1893, 507–518), of which (3) is a restatement, objects that to make imitation the sole social category is a mistake like that of the Ionic school of philosophers with their water, air, etc. "The object of a 'pure sociology' is to determine the conditions apart from which no stable social group is possible." Sociology is social logic, a theory of the inventions necessary for society; and imitation, like language or law, is merely *one* of these necessities. In fact, it falls under the consideration of pure sociology only in proportion as it is shown to be necessary to this end. Professor Simmel attempts a still more definite delimitation. If sociology embraces all that occurs in society, and is understood as an explanation of all events by social forces and considerations, it is only a method (as, *e. g.*, induction), not a special science. But as psychology separates

out the content of mental states and considers simply the form, so sociology must isolate the distinctively social and study the *form* of association as such. The contents, *i. e.*, the interests and objects realized by association, belong to the specific historical and material sciences. The 'form' in question is reciprocal action, association; and in the most various types of social groups—religious, economic, etc.—we find the same special forms of subordination, imitation, division of labor, etc. The mere fact that phenomena are common to all does not make them social, nor do similarities and regularities established by statistics belong here if each has an individual cause. Not what takes place *in society*, but what takes place *by society*, is the field of sociology. M. Durkheim, though his chief aim is to set forth the methods to be employed in the science, prefaces his extremely suggestive articles by a consideration of what is meant by a social fact. A social fact comprises "every kind of activity or functioning, whether fixed in definite law or not, which is capable of exercising an external constraint upon the individual, or one which is general throughout a given society and has a proper existence of its own, independent of its individual manifestations." Language, law, financial systems impose themselves on the individual. They are general because collective (*i. e.*, obligatory), and not *vice versa*.

2. The *relation of sociology to psychology*, implied in these various definitions, is in many cases evident. None of our writers would, I think, accept Mill's statements, "men are not, when brought together, converted into another kind of substance," "human beings in society have no properties but those which are derived from and may be resolved into the laws of the nature of individual man"—*i. e.*, if we understand by 'individual man' man in isolation from all social relations; and it is difficult to find anything worth saying in the statement if it does not mean this. Small, Tarde and Durkheim lay especial emphasis upon society as more than the sum of individuals, and the last named devotes a special criticism to any psychological explanation of social facts—meaning by 'psychological' an explanation based on the laws of the individual consciousness. Similarly Professor Small would distinguish "between (1) *Psychology* which gives an account of mind as we know it in the individual, and (2) *Social Psychology*, which describes the phenomena that result from the combination and reaction of the cognitions, emotions and volitions of associated individuals." Now, I can but think that Mill's 'individual man' hovers in the background of these statements, and that if psychology is limited to the study of that mythical being, its field is so narrow as to be scarcely worth tilling. No doubt the

sociologists are leading the psychologists into broader horizons; but if we follow out the implications of the authors cited, and abstract from 'mind as we know it in the individual,' all that results from the forces mentioned under (2), *e. g.*, all the effects of language and intercourse, all feelings of sympathy, love, all volitions involving others, we should certainly have a much smaller man than even present psychology studies. Would not the 'difficulty of demonstrating the existence of social as distinguished from individual knowledge, feeling and willing, be removed by the recognition here of what is elsewhere implied in the authors' conception of man, viz., that neither (1) nor (2) deals with anything but an abstraction. There is no 'individual mind,' *i. e.*, mind not under the constant influence of the social relations; neither can we hope to find any 'social' knowledge or feeling, *i. e.*, knowledge or feeling not existing in the medium of individual consciousness. We may doubtless study various aspects of consciousness in isolation, but their abstract character should be distinctly recognized, and neither 'psychologist' nor 'sociologist' can ignore the work of the other.

3. *The methods* to be used in investigation are treated at length by Durkheim. They include (1) rules relating to observation of social facts. These are: (a) consider social facts as things. Bacon's 'idols' all find their counterparts in social science at present; (b) in defining and grouping phenomena, such as crime, the family, etc., use at first external marks only, not by what may be deemed the essential characteristics, nor should merely one type of facts be selected and the rest tested by this standard; (c) study especially the consolidations of social facts in law, proverbs, modes, etc. (2) Rules for distinguishing the pathological from the normal. Here the rather startling proposition is advanced that, at the outset at least, the only objective criterion for the normal is the general. Hence normal=the mean, diseased=the exceptional. But it is evident that greater frequency must ordinarily be due to superiority, to 'health;' hence (a) we may control our results by seeking the cause of the generality of a given phenomena in its relation to the general conditions of life in the social type considered; and (b) this becomes especially necessary in the case of a social species in a transitional stage. (3) For making the classification into social types, the objective principle to be adopted as our standard is that of simplicity—*i. e.*, we ask whether a given group is made up of units which enclose other units more simple than itself. (4) Rules for explaining social faults. (a) It is common to find the reason of a fact in its utility. This is to confound final with efficient causation, and

is no more admissible here than in natural science. Function and cause must be examined separately. (b) The cause of a social fact must always be sought in preceding *social* facts, not in states of individual consciousness [see remarks under 2 above]. (c) The function of a social fact should be sought in the relation it sustains to some social end. (d) The first origin of every social process should be sought in the constitution of the internal social medium. This will depend upon two factors: (α) the number of social units, the '*volume*' of society; (β) the degree of concentration, the 'dynamic density,' which is a function of the number of individuals who are in commercial and social relations. If we do not adopt this plan we are reduced to explain progress by 'tendencies' instead of by real causes; and, further, are forced to treat all as one species in greater or less stages of advancement. (5) Methods of induction. (a) The doctrine of plurality of causes bound up with Mill's philosophic presuppositions is to be rejected. The same effect is *not* produced by different causes. (b) The most valuable of the inductive methods for our purpose is that of concomitant variations, for which we may draw our facts either from a single unique society at different times, or from several societies of the same kind, or from several of different kinds.

The principle most likely to challenge criticism is that reducing the normal to the general. M. Tarde has already criticised (*Revue Phil.*, Feb., '95) Durkheim's own inference from it: that crime must be normal because general. But are not both D. and T. hasty in asserting that the above definition carries with it the inference that crime is normal? Is crime ever so general in any group as to be the rule and not the exception—meaning by crime, of course, acts considered criminal by the group in question?

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NEUROLOGY.

Anatomie des Centres Nerveux. J. Dejerine, avec la collaboration de Mme. Dejerine-Klumpke. Vol. I. General Methods of Study, Embryology, Histogenesis and Histology-Anatomy of the Fore-Brain. With 401 figures. Paris, Rueff et Cie., 1895. Price, 32 francs.

The revolution in the doctrine on the architecture of the nervous system has been followed by a number of publications which endeavor to do justice to the rapid progress. None of those books reaches in breadth of plan and in the number and choice of illustra-

tions the new work of Professor Dejerine, the first volume of which has just come out. Professor Dejerine, a pupil of Vulpian, is in the midst of a brilliant career as a clinician in the field of nervous diseases. Independent of Charcot's school, he has done remarkable work in the Hospital of Bicêtre, which is probably without rival in rare nervous affections. His wife, Madame Dejerine-Klumpke, of American birth, has become known through her experimental work in Vulpian's laboratory and through her monograph on polyneuritis. The volume before us is the first of a monumental work on the nervous system from the hands of clinicians, and will for this reason be of the greatest intent and practical value.

The first part begins with the history and description of the methods used for the study of the nervous system (pp. 7–57). A very interesting discussion of the choice of a post-mortem dissection, especially in pathological brains, with full description of the author's own method, precedes the notes on hardening, embedding, staining and drawing. The second chapter (pp. 58–153) deals with the development of the nervous system, and contains valuable teratological remarks, besides a remarkably clear and well-illustrated account of the assiduous work of the last years. The third and fourth chapters (pp. 134–232) are devoted to histogenesis and to histology of the central and peripheral nerve elements in the adult. The first part is profusely illustrated with drawings taken from the publications of His, Retzius, Cajal and Ranvier; the selection of the drawings and their execution deserve equally high praise.

The second part of the volume treats of the anatomy of the Fore-Brain. The first chapter (pp. 223–386) covers the general morphology, the convolutions and fissures, the base of the fore-brain and the configuration of its interior. Numerous drawings from photographs take the place of the customary diagrams. The second and third chapters (pp. 387–666) are practically a description of macroscopic and microscopic serial sections through the cerebrum, made in different directions, and forming the most complete atlas of those parts published so far. A chapter on the cerebral cortex (pp. 667–741), and one on the white substance of the cerebral hemispheres (pp. 742–810), form the end of this first volume, rich in pathological observations with regard to the association systems. The second volume will bring the description of the remaining parts and a systematic analysis of the fibre tracts.

The style is very clear, the current epitome on the margin of the pages very convenient. Schematic drawings are avoided; the figures are very accurately drawn and clear.

Thus we have before us the first half of a work of fundamental importance for the progress of neurology, destined to bring the clinician into closer touch with the anatomical literature that is scattered in monographs and journals, and is here for the first time made satisfactorily accessible. Meritorious as are the smaller works of Edinger, Obersteiner Féré, Debierre, and the purely anatomical treatises, none of them could give such a full account of the minute details that interest the clinicians and pathologists of to-day. It is to be hoped that the second volume will soon follow and bring a very accurate index.

ADOLF MEYER.
KANKAKEE, ILL.

The Growth of the Brain in Men and Monkeys, with a short Criticism of the usual Method of stating Brain-ratios. A. KEITH. Journal of Anatomy and Physiology, Vol. XXIX, Part II, Jan., 1895.

The novel data in this paper comprise a series of brain-weights from 135 catarrhine apes. These have been collected from the literature and carefully compiled in tables. For reasons given in the text, brains preserved in alcohol are considered to have lost about 33 per cent. of their fresh weights. Where the brain-weight was deduced from the cranial capacity, the capacity in cubic centimeters was taken as equal to the same number of grammes. There is evidence that the brain much more nearly fills the cranium in these apes than it does in the case of man, and this method introduces no very great error. To be compared with these data are those on the growth of the human brain, based on the figures by Boyd.

The period during which the brain grows is much longer in man than in apes, even though the author is inclined to limit the growing period in man to twenty years. The difference between the brain-weight at birth and at maturity in man is greater than in the apes, or other mammals which have here been studied. This is but a different expression of the fact that in man the brain grows for a longer period. The central nervous system is precocious in its development, and attains nearly its full weight, while yet the body-weight is little more than one-third that of the adult. In all mammals this precocity of the central system is evident, and is most marked in the cephalic subdivisions of it. All these features are emphasized in man. From a comparison according to sex of the data on the brain weights of the several groups of apes, it appears that just as in man, the male has a heavier encephalon.

This is a most interesting discovery. Our author then attacks the difficult problems of the relation between the mass of the body

and that of the central nervous system. He seeks the determination of the 'corporeal concomitant' or the number of grammes increase in brain for each kilo. of increase in body weight.

Assuming this to be a valid relation, the entire 'corporeal concomitant' of the adult man would be but a small fraction of the total weight of the central system. The explanation here offered is questionable, as is also the explanation of the difference in the weight of the nervous system due to sex; making it dependent on the absolute increase in this corporeal concomitant in the heavier male. Detailed tables accompany this paper. H. H. D.

VISION.

Ueber die Anzahl der unterscheidbaren Spectralfarben und Helligkeitsstufen. A. KÖNIG. Ztsch. f. Psych. VIII, 5, 375–380.

Professor König reproduces with more detail the result which he had already announced—that the entire number of different color-tones perceptible in the spectrum by the normal eye is about 165, and that the entire number of distinguishable brightnesses, from the threshold of sensation to the greatest brightness attainable under the conditions of the experiment, is, for white light, 572. The method is a simple one for the mathematician, and the only wonder is that it has not been applied before. If λ and $\delta\lambda$ are the wave-lengths of two monochromatic lights which are just distinguishable from each other in tone, then $\delta\lambda$ is a variable whose value depends upon the value of λ. Within an interval of the spectrum for which λ changes by any fixed unit, the number of distinguishable color tones would be $\frac{1}{\delta\lambda}$, and the entire number throughout the spectrum would be

$$\int \frac{1}{\delta\lambda}\, d\lambda,$$

taken between the limits λ 430 and λ 655, beyond which, in either direction, the color-tone does not change. The value of $\delta\lambda$, at short intervals throughout the spectrum, has been experimentally determined by Uhthoff (*Gräfe's Arch.*, 34, 4, 1); hence the required curve can be laid down and integrated by graphical means. The number of perceptible degrees of brightness is determined in the same manner from the experimental data furnished by König and Brodhun in their investigation of Weber's law (*Sitzungsber. d. Berl. Akad.*, 1888 and 1889); in this case it is necessary to produce the

curve symmetrically somewhat beyond the part which can be laid down from actual measurements; but that can safely be done, because the area taken in by this means is only a small fraction of the whole area, and has therefore only a small effect upon the result. The entire number of discernible brightnesses is in this way found to be about 660. These results differ very little for the eyes of different normal individuals. For Brodhun, who is green-blind, the differences in sensitiveness to change of brightness were within the limit of probable error; in color-tone his number of distinct sensations is about 140. This number does not differ much from that of the normal eye, for the reason that, although his spectrum does not change beyond λ 550, he has a keener sense for change of color-tone in the blue-green region than has the normal individual.

In this connection it may be interesting to point out a most extraordinary statement which occurs in a book which is otherwise an admirable example of good scientific method—Havelock Ellis' *Men and Women.* It is there stated that Newton was able to distinguish seven different colors in the spectrum, but that most people since his time have only been able to see six. To how many of the 165 colors which are actually discernible by the ordinary person, it may be desirable to give a separate *name* for popular use is a question the answer to which may change from time to time; but not to be able to distinguish between this question and the question of the number of colors which can be separated in *sensation*, is to have a mind which is abnormally incapable of drawing distinctions.

The question of the number of differences of saturation which are just perceptible for different colors has been treated by Aubert, Woinow and J. J. Müller; but, of course, the subject lacks all interest when the investigation is not made with spectral lights. To determine by a corresponding method with the one here considered, the total number of distinguishable sensations caused by light of all kinds would require in effect the integration by mechanical means of a solid body in space of four dimensions—what it is not beyond the powers of the mathematician to accomplish.

BALTIMORE. C. LADD FRANKLIN.

Die Wahrnehmung von Helligkeitsveränderungen. L. W. STERN. Zeitsch. f. Psych., VII, 249–278. 1894. *Nachtrag* to the above. Ibid, VII, 395–397.

Die Wahrnehmung von Bewegungen vermittelst des Auges. L. W. STERN. Zeitsch. f. Psych., VII, 321–386. 1894.

A number of researches have recently been published (by Scrip-

ture and Preyer, in Zeitsch. f. Psych., VI and VII) upon the manner in which variations in the rapidity with which a stimulus changes in intensity affects in various senses the perceptibility of the change. In the first of the above papers, with its *Nachtrag*, Stern investigates the same problem with reference to changes in brightness. After summing up the results of previous investigations, which have had to do mainly with the problem of the upper limit, or the conditions under which rapidly successive changes in brightness yield a continuous impression, he describes in detail his own researches as to the lower limit, or the conditions under which a slow (or a single) change in brightness is just perceptible. For these experiments he used a dark box, having on its further inside wall a round white field, which was illuminated by light thrown exactly on it through a lens, and apparatus for increasing or diminishing in measurable amount and rapidity the light passing through the lens, without altering the exact coincidence of its image with the white field on which it was thrown. His results, which he regards as only provisional, are as follows: 1. When the brightening is approximately instantaneous, and is at once perceived, the relative sensitiveness to change of intensity is constant; Weber's law is valid. He found this relative sensitiveness to change to be about $\frac{1}{30}$—not so fine as sensitiveness to simultaneous differences. 2. If the objective change lasts for a short time before it is perceived, the results as to relative sensitiveness and as to duration of objective movement before perception of change, are distinct: (a) The absolute rapidity of brightening remaining constant, the duration before perception of change is greater the greater the initial intensity; within a certain range of intensities the sensitiveness to change remains constant. (b) The relative rapidity of change remaining constant, the duration is shorter, the relative sensitiveness greater, the smaller the initial intensity (and thus the absolute rapidity). (c) The initial intensity remaining constant, the duration is longer and the relative sensitiveness greater, the smaller the absolute rapidity. (This does not apply to sensitiveness in case the perception of change is instantaneous.) 3. Other things equal, changes in brightness are more rapidly perceived and the relative sensitiveness is greater in indirect vision than in direct. 4. The relative sensibility is less ($\frac{1}{2}$ to $\frac{1}{4}$) when the changes become perceptible only after an interval, than when they are perceptible at once. 5. The reaction time in perception of gradual change in brightness is of considerable length (.4 to .7 sec.). Stern believes that the instantaneous perception of change in brightness is different from the perception of change through

comparison of two phases; both often coöperate, but can also appear separately.

Stern's second paper is a monograph on the question of the visual perception of movement. It consists of four subdivisions. The first (pp. 322–328) discusses the (1) facts—upper and lower limit, difference between perception of phases of movement and of the movement itself, perception with resting and moving eye; (2) the characteristics of movement-perception—influence and recognition of direction, of rapidity, of duration, of presence of objects at rest, the sensitiveness to difference for and the attention to moving objects, and the relativity of movement; (3) perception of movements with different portions of the retina; and (4) visual illusions of movement. The second division (pp. 328–341) gives a historical summary of theories. The third (341–352) announces the results of the author's own observations and experiments, showing (1) a confirmation of Exner's observation that the sensitiveness of the retina in the peripheral portions is greater for moving than for resting objects; and that this is due to irradiation, for both irradiation and sensitiveness to movement diminish by diminished illumination; and (2) that after-images of movement appear when the eyes are closed as well as when they are directed upon some resting object, after observing the movement; they are both positive and negative if the eye has been fixated during an appreciable time, positive only if the eye has not been fixated during observation, or has been opened for a fraction of a second only. The fourth division (353–385) discusses the theory of the visual perception of movements. Three theories as to the sensory factors in such perception are evidently possible: (1) that they consist of several successive impressions of the object at rest—the different phases of the movement—and that the fact of movement is inferred from these; (2) that a sensation of movement exists, specifically different from other sensations, fully elementary and unanalyzable, like color or tone (held apparently by Exner, James, etc.); (3) that a single sensory impression, obtainable in a single instant, can yield the perception of movement; but that this sensory impression, instead of being unanalyzable, consists really of a particular complex of sensations from retina or eye-muscles, or both. In other words, it is a sensory group, forming a 'fusion' which is introspectively unanalyzable, but is really separable into elementary sensations of color and of muscular contraction, with differences in intensity, quality and spatial relations. The term 'sensation' is applicable only in the sense in which it is used in expressions like 'sensation of depth, of smoothness,' etc. Stern rightly

rejects the second of these theories and accepts the other two. He finds that when the sensory impression is momentary, admitting of no comparison of phases, it may be of three varieties: (a) 'changed stimulation'—change in intensity or kind of stimulation in a given region of the retina may under certain conditions give the perception of movement; (b) the 'after-image trail' (Nachbildstreifen)—in case of movement of a certain rapidity, the after-images of previous phases are still present when a new phase is reached, and form a series of images of the same object, running into one another, and forming a complete scale of intensities. Thus different phases of the movement are seen simultaneously. Though we are not ordinarily conscious of these after-images, yet they give to the sensory complex its particular nuance. Changed stimulation can inform only as to the fact of movement; the after-image trail can inform also as to what the object is, its direction and rapidity, and makes possible also the simultaneous perception of several movements in different directions. (c) Eye movements. These three principles, together with the two varieties of comparison of phases (optical and muscular), make five sensory factors, any one of which may give the perception of movement, but which also form varied and complicated combinations with one another. Stern, in conclusion, applies these principles to the explanation of certain complicated perceptions and illusions: the reversibility of the impression of movement, the relativity of movement, the rapidity and after-images of movement.

Stern nowhere discusses the part played by memory-images of other sensory factors, when any one of the above-named factors arouses the perception of movement. He claims, for instance, that the Nachbildstreifen alone, independently of eye-movement sensations, can give the perception of movement. It is true that this may be the only present sensory factor. But the complete perception of movement must be a very complicated affair, the gradually perfected product of a great amount of experience of coöperating sensory factors; and may it not be that the memory-images of sensations from eye and other active bodily movements form a prominent and necessary factor in every perception of movement? If so, then the Nachbildstreifen or other singly present sensory factor, even when it arises from several simultaneous movements in different directions, would receive its perceptual interpretation as movement only through the admixture of such memory-images.

Furthermore, to the reviewer, it has seemed as if there might be a sixth sensory factor sometimes operative, especially in case of the

after-images of movement. When the eyes have been exposed for a length of time to a flickering light, the flickering sometimes continues for a period after cessation of the stimulus; and might be explained as a continuation of the rhythmic adjustment which the retina during stimulation is making to the rhythmically changing intensities of stimulation. Now in case of prolonged observation of a movement, as of a revolving spiral or a waterfall, etc., such flickerings practically proceed in waves along the stimulated portions of the retina, and may possibly continue in the same or opposite sense on cessation of the stimulus, and be a factor in the positive and negative after-images of movement, which Stern attributes wholly to the Nachbildstreifen.

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MEMORY AND ATTENTION.

Experimentelle Untersuchungen über das Gedächtniss. W. LEWY. Zeitschr. f. Psych. VIII, 231–272.

Of the two groups of experiments here recorded, the first relates to memory (retentiveness) of small visual distances (20–200 mm.). By the use of an appropriate 'Augenmassapparat' on which the 'normal' distances were each exposed for 5 secs. in arbitrary order and by application of the method of average error, it was found that the error increased, in general, with the time of retention (1–60 secs.), with exclusion of ocular movement, with distraction of the attention during the interval of retention, with shortened exposure-time, and when two distances for retention were taken in succession instead of one. The most striking exception was that one second for retention was much less favorable than two, due, in L.'s opinion, not to oscillating attention, but to the necessary haste. Other variations suggesting periodicity in the clearness of the image, he inclines to attribute to causes as yet unknown. The memory was not appreciably improved by practice.

The second group of experiments deals with the local memory of simple touch-sensations. The area of stimulation was a limited region on the dorsal side of the arm above the wrist, the method of measurement again that of average error. Under the most favorable conditions it was found that the error steadily increased with the time-interval for retention; was more frequent and greater in a distal than in a proximal direction; was less when estimated with reference to the point ultimately judged right than when referred to the point first selected; curiously varied, being even in one case

less, when the retention-interval was 'filled.' The slight influence of mental reckoning compared with its marked influence in the case of small visual distances, indicates the importance in reckoning of ocular movement. Accuracy of localisation was improved with practice, with variations, however, not easily explained. The introspective evidence, which is well presented as supplementary to the bare record of the tables, shows that the feeling of defective attention by no means always coincides with greater error in localisation, while in the first group it shows how manifold and individually varied are the factors which determine the accuracy of even a comparatively simple act of recognition.

The Relation of Attention to Memory. W. G. Smith. Mind, N. S. IV, 47–73. January, 1895.

An experimental study from the Physiological Laboratory at Oxford! Under suitable conditions sets of letters were exposed each for 10 secs.; the reagent then repeated what he could remember. While memorizing, the attention was variously distracted. Finally, experiments were made for 'normal' results. Comparison of the cases showed that the greatest disturbance was caused by the activity involved in summation (arithmetical progression), and that that produced by speech (repetition of a syllable) was greater than that produced by mere muscular movement (tapping with the finger). The results emphasize afresh the importance of the motor factor in memory, particularly in the suggested interference of the articulatory innervations involved in memorizing by the activity of the vocal mechanism. In the relatively large number of errors of insertion and displacement in the group where distraction was greatest, the author sees the influence of inattention not only to cause fewer ideas to be recollected, but especially to confuse and derange the associative connections: this against Münsterberg. Inattention disturbs the apperceptive process, tends to turn *Wahrnehmung* into *Empfindung* and to produce a sort of *Seelenblindheit;* the essential fact in attention, on the other hand, is 'the strengthening of an idea or impression by the processes of blending and redintegration.' Contiguity in association is merely formal; the real causes are dynamic factors, such as Attention and Interest, and these mainly in connection with motor agencies.

The author introduces a novel 'positive' method of measuring the accuracy of reproduction, viz., by marking it on a certain scale like an examination-paper; but it does not appear to what extent the 'negative' method of error, which he also uses, is thereby con-

trolled. The paper as a whole represents a noteworthy transition from the experimental study of the time-relations of memory begun by Ebbinghaus to the more difficult experimental study of the analysis and dynamic relations of its constituents.

What do we mean by Intensity of Psychical States? F. H. Bradley. Mind, N. S. IV, 1–27. January, 1895.

That sensations are not measurable quantities, that "our judgments of more intensity can be expressed without the hypothesis that more units have been added to a growing sum" (James), is an opinion now widely prevalent as the result of innumerable discussions of Fechner's psycho-physical formula. Such is not the opinion of Mr. Bradley. He holds that not sensations only, but psychical states generally, have quantity in all manner of respects, are in principle measurable, and therefore, since degrees not resting on units are meaningless, do, in some sense, imply the hypothesis which the above quotation rejects. The argument, which is too subtle and elaborate for brief reproduction, admirably succeeds in its professed object of raising doubts. In the exposure of ambiguities, the analysis of aspects, in fact, the whole dialectical 'business' of discovering and considering difficulties, Mr. Bradley shows his unrivalled skill: there may be much more to say on the points noticed, it would be hard to name any point essential to the discussion which has been overlooked. The article is too important to be passed without comment, especially by psychologists committed to an opposite theory, who in propositions like the following, 'the idea of the extended has extension, the idea of the heavy has weight,' etc., will no doubt find matter for explosion. On the other hand, the views which Mr. Bradley recommends would seem to have little bearing on the actual measurement of states of consciousness, so far, at least, as they are considered as amounts of psychical existence; for here the units, though they must be assumed, cannot probably be shown. Relativity of strength or amount, distinctions of kinds and scales—these are the leading ideas, the import of which may be seen in the confusion which disregard of them has brought into many a controversy.

An Analysis of Attention. Alexander F. Shand. Mind, N. S. III, 449–473. October, 1894.

The confusion complained of by Mr. Bradley has certainly not been wanting in the controversy as to the effect on the intensity of mental states produced by attention. Attention is said to increase

the strength of sensations and the clearness of ideas, and in general to be connected with predominance in consciousness of the presented content; with which assertions the view that attention is not itself presentable but a 'special activity' of the subject is sometimes associated. Mr. Shand denies the necessity of this supposed connection of attention with increased intensity or clearness in the object: the object may become obscured or even fade out while we watch it—a remark which seems also to deny that the so-called fluctuations of attention need be fluctuations of attention at all. What attention really does is to make us more clearly aware of the object, to make our consciousness of it predominant, or rather, since 'attension' merely expresses the fact 'I am attending,' it *is* this clearer awareness. The changed strength of the object is primarily due to variable concomitants of attention, such as accommodation and 'interest'; attention itself is a distinct process. This process consists in apperceiving a felt content in such sort as to develop a greater awareness of its systematic complexity. Its earlier stages actively condition the later; it also reacts, the duality of consciousness being after all a 'continuum,' on the idea or sensation attended to, so far, namely, as to make that more active in evoking the fusion and association necessary to the further understanding of the object. Thus one element in attention is 'feeling,' immediate awareness of presentation, the other is 'thought' or interpretation. And this process, *pace* Mr. Ward, is as directly felt or experienced as sensation.

That I am or may become aware of the degrees of my awareness, and that attention, in the sense defined, is therefore a fact of experience and not a metaphysical abstraction seems indisputable. How far, on the other hand, it is rightly said to be directly felt, may be a question of terminology; but in asserting this direct feeling of a distinct process as over against the feeling of what are described as its concomitants, *e. g.*, the strain of accommodation, etc., Mr. Shand is virtually engaged in a triangular combat—he opposes not only Ward, but some to whom Ward is himself opposed.

SMITH COLLEGE. H. N. GARDINER.

ETHICAL.

La Sanction Morale. F. PAULHAN. Revue Philosophique, Mars, 1894, pp. 267–286, and Avril, 1894, pp. 395–419.

According to the author, the Moral Sanction is the logical consequence of responsibility. Responsibility is merely a certain apti-

tude for the sanction. The moral tendency of the first is satisfied by the second, in much the same manner as the tendency of hunger is satisfied by the act of eating. To regard a sanction as logical and moral, implies a logical and moral world perfectly systematized, and which is realized in our world in proportion as that world is moral and logical. The consequences of the acts of an individual, which constitute this moral sanction, may affect the individual himself if the personality regarded as a whole is responsible, or merely the tendencies which have determined these acts; but always in a way calculated to lead to a more complete systematization of the whole, an element of which experiences the sanction. The only end and the only justification of punishment or of reward, the essential elements of the sanction, are the elimination or prevention of evil, the furtherance or the development of the good. Pleasure and pain are signs of the sanction rather than the sanction itself. Repression of evil is in itself a sign of disorder; in a perfect world, devoid of evil, punishment would be wanting and the sanction would consist merely in the preservation of the organism.

As regards partial and impartial responsibility and the corresponding sanction, inasmuch as there is always a certain solidarity between the different parts of a moral person, the responsible element can be reached ordinarily only by acting upon the individual or upon another element. The more this element is systematically associated with the individual, the more the sanction applied to the individual will result in coördinating the elements of the ego, so that they in turn will exercise their combined influence upon the responsible element, and consequently the more the reward or punishment of the individual will be just, and will have the character of a moral sanction. However, inasmuch as the complete coördination, or total incoördination of the psychical elements are purely theoretical cases, no sanction affecting the entire personality is ever applied either with absolute justice, or absolute injustice.

The general rules of the sanction apply also to the diseased, to the insane as well as the sane. So far as parts of their minds, some tendencies, may still afford some degree of coördination, they may be the object of a moral sanction. In the great majority of cases the sanction should not be applied to the whole of the personality; consequently tribunal intervention would be uncalled for. The sanction applies vigorously to criminals who are called insane only because they lack altruistic, or moral feelings, provided the coördination of their acts and feelings is complete and consistent in all other respects.

The sanctions of the social organism are akin to the sanctions of

the psychical, and the two follow a similar process of development, in which three distinct stages may be noted. The first and inferior form of sanction is automatic in character, and arises where the systematization is but slight; the final form is also automatic, but superior in this that the systematization is far greater. There is an intermediary stage of a conscious form of sanction, where preparation is made simultaneously or successively for the superior automatism. This is accomplished by the growth and synthesis of the new elements which are to enter into the final stage, and by the coördination of the acquisitions already made, and also by the more active intervention of the social ego which has not regularly interposed in the first stage, and which in the final one is represented by the general solidarity of the elements of the system. The sanction is more perfect in proportion as the good is more simply encouraged and the evil more simply restrained, and the greater, the precision, without employing intermediaries merely to apply the sanction. The penal sanction is therefore indirect and incomplete. The perfect natural sanction, without intention of reward or punishment, is the best and highest form. F. Paulhan, in short, regards the moral sanction as a particular case of natural selection, in the conservation of the good, and the elimination of the evil. It seems to me, however, that the conscious feature of the intermediate stage in the development of the moral sanction from inferior to superior automatism, implies a voluntary psychical factor which is quite foreign to natural selection. In this stage would appear reflection, deliberation, conflict of desires, the various manifestations of moral feeling, etc., all of which must either be explained away, or their importance unduly minimized if F. Paulhan's account is to stand. Moreover his designation of sanctions as just, or unjust, introduces ideas whose metaphysical implications are incompatible with a naturalistic ethic.

Origines et conditions de la moralité. Pioger. Revue Philosophique, Juin, 1894. Pp. 634–656.

The author contends that there is a difference between a theory of morals and moral practice. The latter antedates the former and gives form and content to it. The early moral, and religious practices as well, have preëminently a social character. Even the conception itself of moral good implies the conception of a social good. Morality is not merely a matter of the intention; and even if it were, the intention must be regarded as having very complex antecedents, and as a part of an extended system showing a solidarity in the act and in the intention, and a still more complex solidarity

of act and intention taken together. Morality is rather the product of sociability, producing social instincts, appearing in the form of customs, moral practices, and finally a theory of morals. As health is the harmony of organic functions, morality is the harmonious manner in which the reciprocal relations of social beings is established. Immorality is social disease.

There is no intuitive moral law before experience, for there is experienced morality before the consciousness of moral law began to dawn. This is abundantly established by the marked sociability among animals and an unbroken line of development from the social instincts of the lowest order of animals to the reflective morality of the most highly civilized man. Morality is, therefore, the result of the social and moral evolution of the race. The unconscious solidarity of social animals, the morality of the Fuegians and Tasmanians, and the advanced morality of our age, all have the common elements of reciprocity and mutual dependence. Even in the life of to-day conscious morality is the exception, and automatic, instinctive morality is the rule. Reflection, questionings of conscience, deliberations of the will, all mark a derangement of the natural moral functioning. Therefore, since morality is developed, it is necessary to go to the simplest and earliest forms of its manifestation to discern its essential character. Beginnings of morality emerge in the necessities of hunger, defense, reproduction, etc.; this is true also for all social animals. In history morality develops parallel to social organization. The force of obligation and of moral sanction lie in the appreciation of the end — conservation of the species and the race. Intelligence becomes more and more aware of the necessity and advantage of solidarity. Moral sentiments are accounted for, inasmuch as the organic functional character of morality is such as to form an integral part of our social vitality, and cannot be disturbed or inhibited without results disturbing it and the unity of action of all our functions. The genesis of social conscience arises through a differentiation of the nervous system especially adapted to the reception of social excitations, as the sense of sight results from a nervous differentiation especially adapted to receive luminous vibrations. Morality is conformable to the same general laws of differentiation, and of coördination, and adaptation and organization, as our other forms of physiological and psychological activity.

Dr. Pioger, as it will be seen, takes a point of view similar to that of F. Paulhan. His article contains also several unwarranted inferences. While it is true that the lower order of animals, the

morality of the Fuegians, and that of the most highly civilized peoples, may contain the common element of sociability, still that common element represents a minimum which is quite inadequate to express completely the essential features of morality. Prof. Edward Caird's contention in reference to the evolution of religion has a similar application to ethic: "that we must read development backward and not forward, and that we must find the key to the meaning of the first stage in the last; for to trace a living being back to its beginning, and to explain what follows from it by such a beginning, would be simply to omit almost all that characterizes it, and then to suppose that in what remains we have the secret of its existence." While morality may be necessarily concerned with social relations, it may quite as well give law to qualify and transform these relations, as that these relations should determine wholly the character of the emerging laws. That moral practices existed before formulated laws does not prove that the laws were not existent, implicitly at least; nor can it be inferred that the laws were merely a classification or a summation of these practices.

The Method of Idealist Ethics. H. MELLONE. Philosophical Review, January, 1895. Pp. 47–64.

The most fundamental ethical question is the following: What is the supreme Ideal of human life? An answer to this implies an answer to other questions dependent upon it,—the meaning and significance of moral authority; the ultimate criterion of morality in conduct, the connotation of the conception of right; and the proximate criterion, the denotation of right. The question of there being an ultimate ideal is an ontological one; it is the question of the nature and purpose of the individual life. The ideal is regulative, not in the sense of showing us what is to be done, but that *something must be done.* Ideas of the concrete forms of duty come from sociological and historical studies, which, however, belong rather to the sphere of practical or applied ethics. In the analysis of our judgments we find some depending on a standard of truth, but others depending upon a standard of value. The latter from two classes; one, which is formed independently of the will, æsthetic judgments; the other, are ethical judgments depending on a standard of right; that is, on a meaning or purpose in our lives. The motive which prompts us to seek for standards of value in these three respects is experienced under the form of feeling. The standard is felt as an obligatory ideal; in thought, as an ideal of truth; in conduct and character, of goodness; in (creative) art, of beauty. Natu-

ralism or Materialism cannot explain these ideals. In them Idealism finds a key to the nature of the Absolute. Feeling is the fundamental medium of connection and communication between the individual and the universal consciousness. Purposive action is feeling-prompted action. This induces a striving or ἔρως in our nature of a three-fold character, corresponding to the three ideals—truth, goodness, beauty. From the individual point of view this striving is after what is not yet realized, but may be so—what is potentially ours. From the universal point of view, these feelings, as they tend to become supreme, constitute a self-surrender to that which is externally real. A process in time cannot be the ultimate and most fundamental fact in the universe. In so far as the absolute is such a process, or has a history, its essential nature is not manifest. However, it is necessary to keep firm hold on the reality of the time-processes of growth and change in the individual lives for which the ideal may be more or less fully realized. Here we are confronted with the problem : In what sense is time a reality ? Time or change is neither an absolute reality nor an absolute unreality. There must be some *via media* between them, which makes it possible to conceive of reality as a multiplicity of individual, finite, growing lives, immanent in a universal and eternally complete life.

The author's comment at the end is in itself the most appropriate criticism of the article as a whole:—"this is not to attain to a full explanation, but only to begin to see the possibility of one." He is to be commended for his insistence upon a distinct selfhood of the individual, however, immanent in the universal life.

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PATHOLOGICAL.

On some of the newer Aspects of the Pathology of Insanity. W. L. ANDRIEZEN. Brain, Part LXVIII, winter 1894.

Owing to many improvements, to which the author himself has been an active contributor, the silver staining method of Golgi has been brought sufficiently under control to enable the detailed anatomy of the central system to be studied by its aid, and to warrant its application to pathological material.

This paper of one hundred and fifty pages attempts a statement of the fundamental plan underlying the arrangements of the nerve cells in the central system ; the expression of this plan in the cerebral cortex ; an examination of the cortex in the vertebrate series, and by this means the determination of the highly elaborated arrangement in man, and the separation of those parts which are

fundamental from those which have been later added, and which constitute the important distinctions between this most complex cortex and that of lower vertebrates.

To all points of the cortex impulses come in over afferent pathways and from lower centres, and leave by efferent elements whose cell-bodies form part of the cortical layer. In this manner all parts of the cortex, where radiations of sensory pathway are found, become turning points, at which such incoming impulses are transmuted into those outgoing. At some of these points, it appears, that sensations may follow an experimental stimulation of the cortex itself, even though the stimulus be insufficient to give rise to a well marked efferent impulse with its associated reactions, and in this we see, not only a confirmation of common experience, but also warrant for the inference that in the upper layers of the cortex are to be found the structures whose activity is accompanied by consciousness. In confirmation of this general conclusion there is found a greater differentiation of the outer portion of the cortex in the predominantly sensory regions—as shown, for example, by the development of Gennari's stripe in the visual area.

The cortex of man is most different from that of other vertebrates by reason of the great development of the deepest layers of cells. All observations point to these cells as concerned in associating the cortical areas one with another, and we thus have, as contrasted with other vertebrates, an unusual anatomical development in man to be associated with the unusual form of mental activity in man. Passing on to the application of these facts to disease arising from disturbances in the cortex, A. finds in cases of alcoholic insanity a degeneration of the dendrons, and even of the cell-bodies of the larger cells constituting the middle layer in both the Rolandic and occipital areas, changes which, in the first instance, would render the passage of the impulses from the incoming fibres to these cells more difficult; and in the second, when the cell body itself is involved, must modify and weaken the subsequent discharge from it, thus giving a good anatomical basis for the symptoms observed. The changes just mentioned, however, are extreme, and between cells so altered, and those in full health, all gradations may be observed.

Throughout the paper the anatomical point of view is that which has been recently worked out by the more advanced investigations in this line, and need not be here recapitulated. In the course of the argument many matters are discussed, but the paper is most important by reason of the good histological evidence here offered for

the architecture of the cortex, and most interesting to psychologists because of the attempt to correlate this architecture with some fundamental features of brain activity. H. H. D.

La Psychologie de l'Amour. G. DANVILLE. Paris, Alcan, 1894. Pp. 169.

M. Danville (the pseudonyme by which a brother of one of our most distinguished neuro-pathologists conceals his identity), is already known in France by his stories, and by a curious article published in the *Revue Philosophique:* 'Is love a pathological state?' (March, 1893). His present book broaches a question which until now has never been scientifically treated, although the way has been cleared by previous works by Stendhal, Balzac, Schopenhauer, Mantagazza, and Bourget. M. Danville gives us nothing that can be properly called the psychology of love as founded on observation and testimony (which would be a very useful work). What he gives us is a definition of love. Love is (1) a *specific, entity-like motive*, distinct from tenderness, sympathy, pleasure, benevolence, etc.; (2) subject to exclusive systematization, with consciousness of sexual desire toward a definite person of the other sex; that is to say, in less barbarous terms, that real love is monogamous; (3) accompanied by exaltation of sexual desire. Love is not itself sexual desire, the latter is only an accompaniment or an effect; (4) it is accompanied by special mental processes. As a phenomenon of consciousness love may be described in the following way: It forms in each of us, by successive perception of those who awake the sexual instinct, a latent subconscious image which sums up all our preferences; it is for a man an ideal image of the most perfect woman; it is something like Galton's composite image, developed in connection with a particular sense. This image persists through a long time, aided by any daily attention which it may receive. Every normal adult thus possesses within him such a subconscious synthesis which is nothing else than his latent power of loving, to be brought out when any real being approximates its characteristics closely enough to call it into activity. It is a curious and, possibly, new theory; and is the only thing in the book of which as much can be said. A. B.

PARIS.

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The Philosophy of Lotze: the Doctrine of Thought. H. JONES. New York, Macmillan & Co., 1895. Pp. XVI + 375.

John Stuart Mill. C. DOUGLASS. Edinburgh and London, Blackwood & Son, 1895. Pp. XV + 274.

NOTES.

Dr. D. Hack Tuke, the well-known writer on mental diseases and editor of the *Journal of Mental Science*, died in London on March 5.

The current issue of the *Princeton College Bulletin* (Vol. VII, No. 1) is devoted entirely to memorials of the late ex-President James McCosh. It contains articles by President Patton, Prof. A. F. West, and Prof. W. Libbey, Jr., a poem by Robert Bridges, a portrait, and a useful bibliography of Dr. McCosh's writings (over eight two-column pages) compiled by J. H. Dulles.

Dr. Ernst Mach, professor of physics in the University of Prague, has accepted a professorship of philosophy in the University of Vienna, and will direct a laboratory of experimental psychology.

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THE PSYCHOLOGY OF PAIN.

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The theory of pleasure and pain, so ably advocated by Mr. Marshall,[1] certainly represents a widespread opinion among the competent in our department. Among its upholders are Wundt and Höffding, Külpe and Lehmann, Sully and Bradley, besides prominent members of this association.[2] According to this theory, pleasure and pain are not independent mental contents, capable of existing in consciousness alone, but a side or aspect of other contents—a sort of modification or coloring of sensations and ideas. In Prof. James's happy phrase, they are 'mere manners of experiencing' these other contents. Wherever we feel a pain, there we have a sensation or idea, distinct from the pain, with reference to which the pain is felt. Furthermore, we never have a sensation or idea which is not felt with some degree of pleasure or pain. So that in every actual state of mind we are able to distinguish these two sides, the cognitive and the affective. The affective side of a sensation is called its feeling-tone, and feeling-tone is conceived as a necessary attribute, to be classed with quality and intensity.

The following quotations will serve to make the doctrine clearer. Sully remarks that "pleasure and pain do not occur as isolated experiences, but in close connection with presen-

[1] *Pain, Pleasure, and Æsthetics*, London, 1894.

[2] Read before the American Psychological Association at its Princeton meeting.

tative elements."[1] Bradley says: "Without sensation we never have pleasure or pain. Not a pleasure, but something pleasant is what we experience. . . . If we like to apply the term aspect, or side, or moment, these are all open to objection, as metaphors must be. But what they try to say is that . . . . pain and pleasure do not exist apart from sensation, any more than duration or intensity are ever discovered by themselves."[2] And Mr. Marshall holds that pleasure and pain are "differential qualities of all mental states, of such nature that one of them must and either of them may, under their proper conditions, belong to any element of consciousness."[3]

Mr. Marshall calls this the quale theory of pleasure and pain, but it seems to me that a more readily intelligible name would be the aspect theory, and I shall so designate it in this paper. What I propose to do is to inquire whether this theory gives a correct account of a single concrete experience, that of physical pain. And the kind of physical pain I shall especially consider is pain felt through the skin. But the whole question suffers from ambiguity. The word pain signifies not only (1) the feeling we have when the skin is cut or burnt, that which the Germans call *Schmerz;* but also (2) the feeling of displeasure excited in us by this and other experiences, that which the Germans call *Unlust.* Now I am aware that the aspect theory is primarily a theory of displeasure. So far as this is so, I am not concerned with it in this paper. I agree that displeasure is always felt with reference to a content distinct from itself. But the aspect theory regards pain as the highest degree of displeasure, and holds that the pain of a cut or a burn can always be analysed into a tactile or temperature sensation on the one hand and a feeling of displeasure on the other. These are the doctrines I wish especially to examine. That pain usually calls forth displeasure I do not question; but that pain *is* displeasure, and is always felt with reference to a tactile or temperature sensation, is what I am inclined to doubt. I

[1] *The Human Mind*, Vol. II, p. 7.
[2] *Mind*, No. XLIX, p. 2. *Cf.* Wundt, *Phys. Psych.*, 3d ed., Vol. I, p. 509 infra.
[3] *Pain, Pleasure, and Æsthetics*, p. 3.

propose, therefore, to take up the experience of cutaneous pain, and to look at it first from the neurological and afterwards from the introspective point of view, with the aim of ascertaining whether the account of it given by the aspect theory is correct.

I.

The question of special pain terminations I shall not discuss; for, now that Goldscheider has withdrawn his former claims on their behalf,[1] there remains little competent testimony to their existence. I pass at once to the important question of the psychological bearing of the condition known as analgesia. It has been maintained that this condition demonstrates the existence of special paths for pain, and so disposes of the aspect theory: let us inquire how far this is true.

1. Analgesia, as every one knows, signifies loss of sensibility to pain without necessary loss of tactile or temperature sensibility. Schiff produced this condition in animals by sectioning the gray matter of the spinal cord and leaving the posterior columns of the white matter intact; though I believe that physiologists question the rigor of his experiments. Analgesia occurs at a stage in the action of cocaine on the skin, and in that of ether or chloroform on the nerve-centres —the result being that the patient feels the touch of the surgeon's knife without feeling the pain of the cut. It also occurs in certain spinal diseases. In syringo-myelia, for example, pain and temperature sensations are lost, while tactile sensations remain normal. In locomotor ataxia, a frequent symptom is analgesia of the legs; and such patients often get burns or sores without noticing, or even allow a fractured bone to go untreated, because they feel no pain.

But the most striking cases occur in hysteria. "Some women," says Dr. Weir Mitchell,[2] "remain for years without the pain sense. In one case of mine a hysterical paralytic recovered really useful health, and except herself and

[1] *Ueber den Schmerz*, Berlin, 1894.
[2] *Medical Record*, Dec. 24, 1892.

myself no one knows that she cannot be hurt by knife or fire or a blow. The interior organs still feel pain as usual. All other forms of skin sense are as keen as ever. This woman used to hurt or burn herself from want of care; now she has learned to take heed. I have seen many such, but only one other where the general health was as competent. She doubts now whether she would accept anew the natural condition of the pain sense."

In the same address Dr. Mitchell mentions a remarkable case of natural analgesia which deserves to be brought to the attention of psychologists. It is reported by a Georgia physician, Dr. Paul Eve, and is that of a man said never to have known pain. Dr. Eve says he knew the patient personally, and was for years intimate with his family physicians, who often spoke of Mr. A.'s peculiar incapacity to feel pain. Mr. A. was about 56 years old at the time of his death. He was a corpulent man, weighing about 250 pounds, and had been a free liver. He was a lawyer by profession, of good intellect, a man of strong mind and body, and had acquired a considerable reputation as an advocate and politician. During a political campaign, not liking the appearance of a finger which had been injured in an affray, he bit it off himself and spat it upon the ground. He had at one time an ulcer on a toe which resisted treatment for nearly three years; he told his physician at the time, and repeated the statement later, that from first to last it never gave him any pain. He also had at one time an abscess in his hand, involving the whole forearm and arm, which became enormously swollen and threatened his life; the lancet had to be freely used; yet during the whole treatment he said he experienced no pain. He said he felt no pain when his eyes were operated on for cataract; and Dr. Eve says he can vouch for his statue-like immobility during the second operation. Only during his last illness did he complain of pain for a time, but passed into his usual insensible condition before he died. "It is proper to say," observes Dr. Eve, "that Mr. A. was a man of great probity, and never boasted of being insensible to pain."

2. Analgesia, however, is only one among a number of

similar conditions, the so-called partial anæsthesias. Thus we may have the converse condition of tactile anæsthesia, or temperature anæsthesia, or both, without analgesia. Schiff produced this condition in animals by sectioning the posterior columns of the cord and leaving the gray matter intact. It is said to be producible by anointing the skin with acetic or carbolic acid.[1] Finally, it occurs in nervous disease. Mosler[2] has recorded the case of a woman with brain disease whose right side was insensitive to touch, though pain and temperature sensations remained normal. The prick of a pin caused distinct pain, yet she did not feel the laying-on of the entire hand. When a fold of skin was lifted between the fingers and severely pinched, she was aware of what had happened through the pain alone. In a case of locomotor ataxia, also recorded by Mosler, the prick of a pin caused pain everywhere, yet on the left leg the pressure sense was so dulled that the patient could not tell the difference between 100 and 500 grams, nor even feel their weight on the skin.

3. The facts thus far given seem to justify the conclusion that the senses of touch and of pain are independent of each other. But they likewise indicate that the sense of temperature is independent of both. For where pain is lost and touch retained, temperature is sometimes retained and sometimes lost; and, conversely, where touch is lost and pain retained, temperature is sometimes retained and sometimes lost. Indeed, it is probable that the temperature sense must be divided into a sense of heat and a sense of cold; for cases are on record in which cold sensations were lost and heat sensations retained, and other cases in which heat sensations were lost and cold sensations retained.[3]

To sum up: The conclusion generally drawn by pathologists is that the skin possesses four distinct forms of sensibility, namely, touch, cold, heat and pain, and, that any one or any combination of these may be lost or impaired without

[1] Beaunis, *Les Sensations Internes*, p. 214.
[2] *Berliner Klinische Wochenschrift*, 1868.
[3] See Foster, *Physiology*, 5th ed., Part III, p. 1101.

detriment to the rest. The nerve-impulses which awaken these different kinds of sensation are assumed to pass upward by distinct paths in the spinal cord, and the partial anæsthesias are explained in an anatomical way by the blocking of these paths. Whether this explanation is the true one we shall see later on.

Let us at present consider how the aspect theorists meet the facts of partial anæsthesia. They do so in a very simple manner, namely, by holding that the fourth sense is not properly a pain sense, but a sense whose normal product is what Mr. Marshall calls the cutting-pricking sensation, and that it is the exaggeration of this sensation which gives rise to pain. What is lost, according to Mr. Marshall, in analgesia is the cutting-pricking sense with its pain-giving capacity, while the tactile and temperature senses with their pain-giving capacities remain unimpaired.[1]

This seems a not unnatural interpretation of the facts. But when we go on to ask what *are* the pain-giving capacities of the tactile and temperature senses alone, the answer is one of some importance to the aspect theory. For the pain-giving capacities of these senses alone appear to be practically zero. The analgesic will endure a violent blow on the skin, or the contact of ice or of a red-hot iron, without the slightest trace of discomfort. So that, if the aspect theorist still holds that tactile and temperature sensations in themhave an affective coloring, he must at least admit that this selves coloring never amounts to positive pain.

Observe the consequences of this admission. One of them is that, when we get a painful burn, and suppose the pain to be a modification or aspect of the heat, we are under an illusion. For the pain and the heat are called forth by separate nerve-fibres. Or when we feel a painful pressure, and suppose the pain to be an attribute of the pressure, we are under an illusion. The pain can at most be an attribute of the cutting-pricking sensation, if we admit its existence. But what is this cutting-pricking sensation, if we mean by it the feeling of being cut or pricked? Is it not a tactile sensa-

[1] *Pain, Pleasure, and Æsthetics*, pp. 19, 20.

tion, due to the inevitable stimulation of tactile terminations which just precedes the prick or the cut? If Mr. Marshall refers to anything else, I confess he seems to me to be wilfully assuming a new kind of sensation, without sufficient introspective evidence, for the sake of a theory.

It seems to me (speaking from the point of view of the aspect theory) that Mr. Marshall would have done well in this matter had he followed the example of the judicious Foster, whose doctrine his own so closely parallels. Foster[1] agrees with Goldscheider[2] that "we have no evidence that simple stimulation of the retina, however excessive, will give rise to pain—meaning by pain the kind of sensation we feel when the skin is cut or burnt. . . . We have no evidence that an auditory or an olfactory or a gustatory sensation can, through mere intensity, become converted into a sensation of pain. . . . We may assume that the pain which we feel when the finger is cut is a wholly different thing from the pain which is given to the most delicately musical ear by even the most horrible discord." And these considerations suggest to Foster that cutaneous pain is not simply an exaggeration of tactile or temperature sensations, but a separate sensation developed in a different way. But even on the assumption that this difference of pain-giving capacity is not one of kind, but only one of degree, it is surely important to emphasize the distinction between the senses which are normally analgesic and those which are not. These remarks I make hypothetically, from the point of view of the aspect theory, which for reasons soon to be explained I am unable to accept.

Goldscheider,[3] while agreeing with Foster that sight, hearing, taste, and smell are analgesic, thinks it unnecessary to assume a fourth set of cutaneous fibres, and refers cutaneous pains to the sense of touch. He believes that the temperature sense is analgesic, while the sense of touch is not; and bases this opinion on the fact that pains can be pro-

[1] *Physiology*, 5th ed., Part IV, pp. 281, 282.
[2] *Ueber den Schmerz*, p. 8.
[3] *Ibid*, p. 13.

duced by stimulating the pressure spots but not the heat and cold spots. How far this view is a necessary inference from his observations I cannot judge, but there are further facts of nervous disease which seem to contradict both it and the view of Foster, and, indeed, to be quite irreconcilable with the interpretation of the partial anæsthesias thus far given. I refer, first, to a symptom of locomotor ataxia recently put forward by one of Prof. Starr's assistants, Dr. Skinner, and consisting in a hyperalgesia of temperature without accompanying hyperalgesia of touch.[1]

The tests were made by heating or cooling water in a test-tube and holding the tube against the skin. For warmth the water was heated to 120° F. (50° C.), for cold it was cooled to 50° F. (10° C.)—temperatures quite supportable to a normal nervous system, but productive of pain to these tabetics. Out of 24 cases examined, the tests showed the presence of hyperalgesia to both heat and cold in three, and of hyperalgesia to heat alone in one. In one of the three cases the symptoms were very marked, the patient starting and uttering an exclamation of pain when the test-tube was applied to the skin. In this case there was analgesia of both legs to pricking and pinching, and *the hyperalgesia to temperature was present over the analgesic areas, and over these areas only*. The heat was always recognized as heat and the cold as cold. In another case there was no disturbance of cutaneous sensibility except the hyperalgesia to temperature, and this was greater to cold than to heat—so great, in fact, that the patient begged to have the test-tube removed at once, although the temperature of the water was only 58° F. (15° C.).

I may remark in passing that this interesting symptom is not so new as Dr. Skinner supposes. Dr. Bolko Stern mentions it distinctly in an article in the *Archiv für Psychiatrie* for 1886. He noticed in certain cases a manifest hyperalgesia to contact with cold objects, in persons who bore painful stimuli on normal skin-areas with patience. One woman started and cried out on being touched with cold vessels

[1] Starr, *Familiar Forms of Nervous Disease*, pp. 173–175.

which caused no unpleasant sensation to normal persons. Dr. Stern was, however, unable to discover a clear case of hyperalgesia to warmth.

Both Dr. Stern and Dr. Skinner mention that ataxic patients are often very sensitive to cold. Thus Dr. Skinner mentions a patient who could not dress warmly enough for comfort, and who kept his room at a temperature of 90° F. (32° C.), yet still felt chilly all the time; and another who had felt continual chilliness for five years. These cases seem to differ only in degree from those of marked hyperalgesia. They are interesting as bridging the gulf between pathological and normal sensitiveness to cold.

I am indebted to Dr. Charles W. Burr of Philadelphia for directing my attention to a second symptom of nervous disease which, while the converse of the former, seems like it to show that tactile pains are distinct from temperature pains. In certain cases of syringo-myelia, the patient's foot is normally sensitive to pricking and pinching, but totally analgesic to temperature stimuli such as ice or a hot iron.

Here, then, we have facts which seem to show that the paths of temperature pains are distinct from those of pains due to pricking and cutting; and even that the paths of heat pains are distinct from those of cold pains. They likewise seem to show that temperature pains are more closely bound up with normal sensations of temperature than would be the case if pain impulses were conducted by a special set of fibres. But how can this be reconciled with the apparent teaching of the partial anæsthesias?

Possibly it might be by the assumption that these ataxic temperature pains are a pathological exaggeration of the mild and harmless affective coloring which the aspect theorist supposes to attend the functioning of the temperature sense alone. But when we consider that this would oblige us to explain the pains of the ataxic by one sense of fibres, and those of the normal person when the test-tube is heated a few degrees hotter by another set, the explanation appears somewhat forced and unnatural. We are moved to return to the partial anæsthesias and attempt a physiological explana-

tion of them, by assuming that tactile and temperature fibres are each capable of carrying two kinds of impulses, ordinary impulses and pain impulses, and that either of these capacities may be lost as a result of disease. But this explanation is promptly negatived by the experiments (if we may trust them) of Schiff, which appear to prove that moderate impulses pass upward through the white matter of the cord and excessive impulses through the gray matter.

An hypothesis in regard to analgesia, which promises to help us out of our difficulties, was published by Wundt in the first edition of his *Physiologische Psychologie*, and has been repeated in subsequent editions.[1] Wundt assumes that in the peripheral nerves the paths of pain impulses are the same as those of touch, heat and cold impulses. He sees no reason for assuming a special pain sense, or a fourth sense of any kind. But he thinks that, when tactile or temperature impulses reach the cord, they find two paths open—a primary path, probably leading through the white matter, and a secondary path or paths, leading through the gray matter. Impulses of moderate intensity take the primary path, and this path can accommodate only moderate impulses. When excessive impulses come, they overflow into the secondary paths and pass upward through the gray matter.

This hypothesis, which has the countenance of Funke and of Goldscheider,[2] will be found to explain most of the above-mentioned facts. Hypnotic analgesia, for instance, may be due to a temporary, hysterical analgesia to a more permanent, lowering of the excitability of the gray matter, the effect being to block the paths. Observe, first, that this hypothesis provides more or less distinct paths in the gray matter for tactile pains and for temperature pains. That these paths should be entirely distinct is not required by the facts; it would suffice if the two kinds of impulse entered the gray matter at different points or passed to different centres, which could undergo lesion separately. Observe, secondly, that the hypothesis does not exclude the

[1] 3rd ed., Vol. I, pp. 115, 116; 4th ed., Vol. I, pp. 111, 112.
[2] *Ueber den Schmerz*, p. 19.

existence of a fourth cutaneous sense: although it invalidates the arguments for it drawn from the condition of analgesia. And, of course, if such a sense be assumed, it is no longer necessary to distinguish between its pain-giving capacity and that of the other cutaneous senses; since analgesia is now explained by the blocking of the excessive impulses which enter the gray matter, not by the abolition of this fourth sense.

Finally, it must be admitted that this hypothesis is not free from difficulties. It requires us to explain the action of cocaine and that of acetic and carbolic acid on the skin by a differential paralysis, now of the pain-carrying capacity of the nerve-fibre, and now of its touch-carrying capacity. The hypothesis also seems inconsistent with the fact that pain cannot be produced by stimulating the heat and cold spots; though, perhaps, it could be produced if enough of them were stimulated simultaneously. Finally, it ignores the consideration by which Foster seeks to establish an analogy between touch and the higher senses, though it is not inconsistent with the analgesic character of these senses themselves.

But the question which chiefly concerns us is as to the bearing of all these neurological facts and theories on the psychology of pain. This may be summed up as follows:

(1) The evidence seems on the whole to indicate that pain impulses are exaggerations of tactile, heat, and cold impulses, and are conducted inward by the same nerve-fibres; and this seems favorable to the aspect theory.

(2) The analgesic condition seems to be one of indifference, so far as the remaining cutaneous sensations are concerned, and this seems hostile to the aspect theory. The affective cream seems to be taken cleanly off, leaving a mere skim-milk of cognition. But this, again, may be disputed, and it may be claimed that the currents which pass upward through the gray matter awaken a sensation with a strong feeling-tone, and that this is now lost; while those which pass upward through the white matter awaken a sensation with a weak feeling-tone, and that this still remains.

It seems to me that with this claim we have reached the

limit of what can be learned from neurology. It is evident that the aspect theory does not stand or fall with the existence or non-existence of special pain nerves or of special paths for pain in the spinal cord.

II.

Let us now turn to the introspective analysis of pain, and inquire whether the aspect theory gives a correct account of the subjective facts. This theory regards feeling-tone as an attribute of every sensation, and compares it to intensity. Is the comparison a just one? And, in the first place, is feeling-tone a *necessary* attribute of every sensation? A sensation is unthinkable without both quality and intensity—it must be a definite kind of sensation, and there must be a definite amount of that kind. Is a sensation unthinkable without feeling-tone? So far is this from being true, that a large proportion of our ordinary sensations are practically without it. No doubt most of these are attended with some minimal degree of feeling, but this is so slight as to be unappreciable.

This state of things has been interpreted by the aspect theorists in two ways:

(1) According to Wundt, these apparently feelingless sensations do not prove that feeling-tone is not a necessary property; they have a feeling-tone, but their feeling-tone is zero; it is one of indifference.[1] This is as if one should say that every tactile sensation has some temperature quality, but that when the contact is neither hot nor cold the temperature quality is one of indifference. But temperatures neither hot nor cold are pure contacts; and in the same way sensations neither pleasant nor unpleasant are not sensations with a feeling-tone of zero, but sensations without any feeling-tone at all.

(2) Other aspect theorists admit this, but hold that there are no such feelingless sensations, that all sensations have at least a minimal feeling-tone. To this view I have nothing whatever to say; nor do I see how it can be either substan-

[1] *Physiologische Psychologie*, 3rd ed., Vol. I, pp. 290, 508–9.

tiated or overthrown by introspection. I will merely add two remarks: First, that the demand that all sensations should be accompanied by at least a minimum of pleasure or pain is a theoretical demand, based on the prior assumption that feeling-tone is a necessary attribute, analogous to quality and intensity; secondly, that the recognition of a zero-point is equivalent to the admission that theoretically this attribute is not a necessary one.

Let us now pass to the opposite end of the scale, and consider the case of sensations (to use the Wundtian terminology) with a maximal feeling-tone. The aspect theory holds that in physical pain we always have tactile, temperature or other sensations present 'in painful phase,' 'with a coloring of pain.' Thus Lehmann says that "a feeling, whether of pleasure or of pain, never occurs apart from a sensation however weak, and in every case where such an isolated feeling is supposed to have been observed, the sensational element has merely been overlooked."[1] And Külpe, in his recent book, says: "The characteristic feature of pain is not the sensational quality, which is never absent, whether this be great heat, or strong pressure, or a screaming noise, or a blinding light, but the feeling of the disagreeable, of which pain is the highest degree."[2] Here we have the orthodox Wundtian doctrine. But elsewhere, after observing that pain occurs almost solely in connection with common sensations, Külpe goes on to remark that the analysis of these "is rendered difficult by the fact that strong feelings cover up (*verdecken*) sensational qualities they accompany";[3] and in another passage he refers to "the well-known fact that pain usually overpowers (*übertäubt*) the accompanying sensational qualities," and remarks that "if pressure, heat, or cold is intensified until it becomes painful, the impression is in all three cases of essentially the same kind."[4] With this we may compare the statement of E. H.

[1] *Hauptgesetze des menschlichen Gefühlslebens*, p. 33.
[2] *Grundriss der Psychologie*, p. 93.
[3] *Ibid.*, p. 152.
[4] *Ibid.*, p. 327.

Weber that "the pain produced by heat or cold is very different from the sensation of heat or cold. If the pain is not extreme, we feel at the same time the heat or cold which causes it, and can then distinguish pain due to heat from pain due to cold. But if it is extreme, the sensation is the same, whether caused by heat or cold."[1] Or, as Prof. James tersely expresses it, "heat, cold, and pressure are indistinguishable when extreme—we only feel the pain."[2]

If we attempt to render these familiar facts in Wundtian phraseology, we can do so only by saying that in extreme physical pain the feeling-tone has become so excessive as to overpower and cover up the quality and intensity of the sensation. We appear to have left a sensation with an intense feeling-tone, but without any quality or intensity. And yet these three are supposed to be necessary attributes of every sensation. I know Külpe insists that the sensational quality is never absent, but this directly contradicts his statement that it is overpowered by the pain, and that pain caused by pressure, heat, and cold have the same quality. Indeed, he is himself more or less conscious of the contradiction, for he follows up the statement last referred to with the words: "Here we are of course conceiving pain not as feeling-tone, but as a peculiar quality of sensation."[3] And elsewhere he refers to the possibility that "we have to recognize in pain, apart from the displeasure (*Unlust*) therein contained, a special class of sensation, called forth by the intense stimulation of every sensory nerve."[4]

Now this, it seems to me, is the only doctrine which accurately reflects the facts, and it is incompatible with the aspect theory, which classes feeling-tone with intensity. This classification is illegitimate, because at the bottom of the scale we have quality and intensity without any feeling-tone, or with next to no feeling-tone, while at the top of the scale we have feeling-tone in great intensity, but no quality.

[1] *Tastsinn und Gemeingefühl*, p. 118.
[2] *Briefer Course*, p. 68.
[3] *Grundriss der Psychologie*, p. 327.
[4] *Ibid.*, p. 248.

In other words, what the aspect theorist calls feeling-tone is in reality a kind of quality. *As the quality of warmth becomes intense it becomes mixed with the quality of pain, and when the pain is extreme there is no warmth left.* Only in the intermediate sensations is it possible to draw a distinction between a cognitive and an affective side of the experience ; at one extreme all is cognition, at the other all is feeling. And throughout the scale there is an opposition between the two sides, such that one can predominate only at the expense of the other. Thus Sully tells us that "the affective and the cognitive element do not appear with equal prominence in our sensational and ideational experience" ; that in organic sensation and emotion the affective preponderates over the presentative, while in visual perception and ordinary thought the presentative preponderates over the affective.[1] Is such an opposition conceivable between the quality and the intensity of a sensation? Can the intensity of a sensation cover up and conceal the quality? No more than the breadth of a smile can conceal the face. The quality and the intensity are given with equal prominence, because they are only two different ways of classifying a single unique content. Indeed, so pregnant a fact is this opposition between cognition and feeling, that, if the extremes of indifference and pure pain were not to be met with in experience, this opposition would alone suffice to prove the incorrectness of the theory which classes feeling-tone with intensity.

The aspect theorist may be said to make the same mistake as a person who, viewing the series of colors between red and yellow, should describe the yellow apparently visible in orange as a sort of tone, and the red visible in it as the sensational basis to which this tone was attached. The phrase hedonic coloring proves nothing, but it shows how well the facts would lend themselves to a sensational interpretation.

To make my doctrine perfectly unambiguous, I must distinguish between cases where the mingled qualities are due to different nerve-fibres, and cases where they are due to

[1] *The Human Mind*, Vol. II, pp. 10, 11.

the same nerve-fibre. It is quite probable, even on our theory of the partial anæsthesias, that in the experience of a painful burn the pain is due to different nerve-fibres from the heat. For it is reasonable to suppose that the area of skin actually burnt is surrounded by a margin in which only heat impulses are produced. At the same time, it can hardly be doubted that in cases of moderate pain both the pain and the heat may be due to the same nerve-fibre. Let us suppose a fibre or a set of fibres to be stimulated with such intensity as to cause slight pain, yet not so intensely as to obscure the normal quality of heat. Now, to determine the relation between the heat and the pain in cases like this is, of course, the strict object of this paper. And I submit that this sensation of painful heat is to be conceived as a mixture of two co-ordinate qualities, heat and pain. When I say a mixture, I do not mean to imply a composite character in the sensation itself, any more than the taste of lemonade is composite. But, just as the taste of lemonade resembles that of lemon-juice on the one hand and that of sugar on the other, so the sensation of painful heat resembles the pure quality of heat on the one hand and the pure quality of pain on the other.

My conclusion is, then, that pain is as distinctly the content of certain cutaneous sensations as blue of certain visual ones. It seems to me that Bain is right in classing hunger, thirst, nausea, fatigue, and pain with sensations;[1] that Delbœuf is right when he says that "fatigue and sensation are phenomena of the same nature and comparable to each other"[2]; that Münsterberg is right when he says that "sensible pleasure and pain are not extreme degrees of the agreeable and the disagreeable, but sensations, which regularly call forth strong feelings of the agreeable and the disagreeable"[3]; and that Dr. Nichols is right in so far as he places physical pleasure and pain "on a common footing with our other senses as fundamental elements of mind."[4]

[1] *The Senses and the Intellect*, pp. 102, 103.
[2] *Éléments de Psychophysique*, p. 46.
[3] *Beiträge*, Heft IV, pp. 216, 217.
[4] *Philosophical Review*, Vol. I, p. 404.

Or my conclusion may be briefly expressed in the following form. If a new-born babe were subjected through all its senses to stimulations of excessive intensity for the space of an hour, the sole content of its consciousness during that time would be the feeling of pain, nor would this feeling have any connection with or reference to cognitive states whatever.

III.

In conclusion I will reply to an objection and indicate two consequences.

I am sure some of my hearers will be inclined to admit that pain is an independent mental content, not necessarily attached to any other element of consciousness, but yet will deny that it is on that account to be classified with sensations. They will say that in this classification an important character of pain has been overlooked, one which differentiates it totally from the states we call sensations. Sensations, that is to say, are projected outside the body and elaborated into perceptions of objects, of which they then appear as the qualities; whereas pain is not thus projected or elaborated, but remains throughout a purely subjective state.

It is perfectly true that pain is a subjective state in this sense. But be it observed that such elaboration into objects is a subsequent operation, performed upon the sensation as it were from without, and not necessarily implying an essential difference between it and the feeling of pain simply as mental elements, apart from their differing fate in this respect. The considerations already brought forward suffice to prove that originally and in themselves feelings of pain and ordinary sensations are of the same nature. For both are called forth by nerve-currents from without; and both are substantive mental contents, capable of existing in consciousness alone, though commonly present together. The only difference between them is that sensations are found in experience to vary with their stimuli, and therefore to furnish information about the stimuli, whereas feelings of pain do not so vary, and therefore tell us only of ourselves. Bradley puts the matter clearly and forcibly when he says:

"Like sensations they [pleasure and pain], are at first neither objective nor subjective. . . . That pleasure or pain, as they come first, have, in any sense whatever, a reference to the Ego is a fundamental error. It takes the products of development and places them at the starting-point, where no Ego . . . exists except in false theory."[1] And I could match this passage with another from Prof. Wundt.[2]

The same point may also be stated in the following way. Feelings of temperature are sometimes due, not to external objects, nor even to the contact of the surrounding air, but to the state of the circulation, to the increased or diminished development of animal heat. If they were always due to this cause, if they varied as little with the stimulus and as much with the organ as do feelings of pain, we should account heat and cold no less subjective than we now account pain.

I pass to the two consequences.

(1) It is often said that pleasure and pain are not localized, but that what is localized is the sensations they accompany.[3] But we have seen that often there are no such accompanying sensations, and yet we know that the pain is localized. When I burn my finger, it is not merely the heat sensation that is localized there, it is the pain as well. A person whose hand is stimulated behind a screen may not be able to distinguish the pain of a burn from that of a cut, but he feels the pain in his hand.

(2) It is often said that we cannot have images of pleasure or pain, or that, if we do imagine them, we actually begin to feel these states anew.[4] It seems to me that, so far as this statement is true, it applies to other images as much as to images of pain. The theory that images have a permanent identity and recur, instead of being re-created, may be said to have received its deathblow. If images of pain are faint and unreal, the same is the case with images of taste and smell. I will not assert with Hume that images

[1] *Mind*, No. XLIX, p. 2.

[2] *Vorlesungen über Menschen und Thierseele*, 2nd ed., p. 226 infra.

[3] Sully, *The Human Mind*, Vol. II, p. 7 infra.

[4] Marshall, *Pain, Pleasure and Æsthetics*, pp. 29, 30.

are merely weakened sensations; but I do assert that whatever is true of images generally is true of images of pain.

Finally, let me repeat that my criticisms are directed, not against the aspect theory as a theory of displeasure, but against the aspect theorists' analysis of physical pain. I hold that physical pain is not a compound of an indifferent sensation with a feeling of displeasure, but itself a sensation which calls forth displeasure.

EXPERIMENTAL INDUCTION OF AUTOMATIC PROCESSES.[1]

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That processes analogous to those that normally accompany consciousness do at least sometimes take place in connection with a human brain, without being represented in the consciousness properly belonging to that brain, is now generally admitted; but the full import of the admission to psychology is not clearly understood. In the first place, it is not certain whether these processes are accompanied by consciousness or not. If not, we must suppose that the cortical process, as such, is not alone sufficient to produce a mental state, but needs the coöperation of some other factor. This hypothesis bears too much resemblance to the old soul theory to meet with favor in contemporary psychology, and we find that most writers claim that these dissociated processes are accompanied by true conscious states, which are related to the complex of similar states that constitutes the personal consciousness of the individual much as the consciousness of some other person is related to it. Furthermore, it is supposed that these 'parasitic states' are subject to precisely the same laws as those that govern the ordinary 'upper consciousness' of the individual in question. They may develop, take to themselves associative helpmeets, and finally form a subconscious dream, which may persist for a considerable time and produce sundry disagreeable phenomena in the consciousness of its involuntary host. In extreme cases the mental parasite may become so complex and highly organized as to constitute a true 'secondary personality' in all respects analogous to the original or upper personality; and it may at times displace the upper person-

[1] Read at the Princeton meeting of the American Psychological Association.

ality and assume temporary control of the motor system, thus becoming manifest to other persons. Pierre Janet, who has developed this doctrine more consistently than any one else with whom I am acquainted, regards such parasitic idea systems as essentially pathological phenomena. But a somewhat analogous conception is being developed among those interested in 'psychical research,' according to which all thoughts, memories, hopes and fears which have been dismissed from the upper consciousness still exist in conscious form, and even organized into a self-conscious being in what is termed 'the subliminal self,'—and some think that the 'subliminal self' is sensitive to influences which are lost upon the normal consciousness.

Among the phenomena frequently ascribed to such subconscious automatism are the 'phantasms of the glass' and automatic or 'spirit' writing-phenomena of which the 'professional psychologist' usually knows less than he should. Indeed, so far as I know, no professional psychologist has made any study of the phantasm of the glass from the purely psychological point of view, and but little has been done with automatic writing. I have been conducting some rather desultory experiments in these lines for the past two years, and, although the results are not sufficiently exact for purposes of computation, they have raised in my mind no little doubt as to their supposedly subconscious origin.

Let us first examine the phantasm of the glass. It has been known since the dawn of history—and no one can say how long before—that certain individuals, while looking into a transparent or reflecting medium, see therein hallucinatory scenes and figures which were supposed to emanate from the unseen world of spirit. From the Urim and Thummim of the Jewish highpriest to the 'Crystal' of Dr. Dee, we find this belief among all ages and peoples. Within the last few years the Society for Psychical Research has undertaken to look into this ancient superstition and see whether it be based upon any residuum of fact.

Those who believe in the existence of conscious states dissociated from the normal upper consciousness of the individual, are inclined to regard the phantasm of the glass as

the product of subconscious automatism, brought by the transparent medium within the ken of the upper consciousness; and it cannot be denied that many cases are on record which would seem to point to some such origin. But in my own series of experiments I found no reason to make this assumption. I commonly used in my experiments a glass ball made for the purpose, but I found that a glass of water or a small mirror reflecting a white surface answered the purpose quite as well. Such a medium gives the patient a vision of unfilled space, and its function appears to be simply that of an irritant to the highly organized visual mechanism. This seems borne out by the fact that the phantasm is more likely to appear when the medium is well illuminated. Usually some interval elapses before any effect is produced. In a few cases the phantasm was seen upon the first glance into the medium; more commonly one must wait from five seconds to five minutes. In one case the image appeared after the lapse of twenty minutes. The first symptoms of a response on the part of the central visual mechanism to the exciting stimulus are frequently found in the appearance of visual sensations of a rather indeterminate character. The medium becomes opaque, being apparently filled with smoky or milky masses; sometimes small masses of white, like minute clouds, drift rapidly through it. At other times these prodromal phenomena take the form of flushes of color —red, blue or yellow. More seldom yet, the medium seems to become more brilliantly illuminated just before the phantasm emerges. The phantasm sometimes appears suddenly, but more often is slowly developed out of some of the sense material already present. The cloud-masses take definite shape and then become colored, or the vague blur or spot becomes a nucleus upon which the image develops. One of my subjects, a young girl who visualized well, described it in *naïve* fashion: "You see," said she, "the gray spot seems to sink down to the bottom of the glass and turns and whirls about slowly; then, of course, it has to become something." "But," said I, who do not visualize at all, "why *must* it become something?" "Well," she said, "of course it could not possibly stay a spot; it has to

become something clear or else go out." When there are no indeterminate prodromal phenomena the phantasm sometimes develops out of the reflections on the surface of the glass, but more commonly the reflections interfere with its development, as do all other sensory distractions.

In the case of good 'seers,' the prodromal phenomena appear within five or ten seconds, and the image is fully developed within a minute or two. It is usually brilliantly colored and resembles a minute picture. Sometimes it is indistinct, sometimes the outline is distinct but lacks coloring. Occasionally the picture is perfectly clear and brilliantly colored, but is imperfect. One of my subjects, for example, saw a portion of the full-page cartoon in a recent number of *Puck*, representing the American school system in the clutches of the Popish tiger. The head, forequarters and forepaws of the tiger and a part of his victim were perfectly clear, but the hindquarters were altogether lacking. This patient was a good visualizer, and I asked him to externalize the remainder of the picture and unite it to the image in the glass, but he could not. He said the phantasm had a vividness and externality which images voluntarily externalized never attained. The duration of the image varies, but seems to bear some relation to the length of time it took to produce it. Generally it lasts but a few seconds, but I have seen cases in which it lasted several minutes.

The images are often drawn from the subject's recent visual experience but are often unrecognized. Many of the latter are purely imaginary, but some are doubtless forgotten memories. In one case I found that I could revive memory by hypnotizing the patient and asking him to remember. Once he saw in the glass the face of a young girl which he described in detail. He had never seen her before, he said. Next came a little dog, which he remembered having seen that day in a country postoffice; it came in with its mistress. He could not describe the mistress—he had not seen her face clearly—but was quite sure she was not the woman he had seen in the glass; she was much older. When hypnotized and told to describe the dog's mistress, he described the face he had just seen, and remembered that he

had caught a glimpse of her face as she passed him. At another time he saw a bust of some white material, but it had no pedestal or other surroundings, and he could not remember where he had seen it. When hypnotized he described the pedestal, the stuccoed wall behind it, the wooden floor upon which the pedestal stood, and himself standing before it; but further than this the picture would not develop.

Often the successive images seem to have no relation to one another. When they are related, however, it is nearly always by the law of similarity, seldom, as in the case above quoted, by contiguity. Sometimes it seems as if certain elements in an image persisted and formed the nucleus upon which the new image took shape, thus making it seem as if the images melted into one another. This bears heavily against those who claim that similarity is an ultimate law, and is analogous to the case reported by Mr. Galton in his *Inquiries into Human Faculty*.

The relation of the phantasm to simultaneous sensory states also goes to show that it is a temporary creation and is not the product of independent subconscious processes. Usually it is most intimately related to the sensations which collectively constitute the medium. The least movement of the medium tends to destroy it. The introduction of any visual sensations other than those proceeding from the medium usually destroys it. Magnifying the medium usually destroys it. Upon closing the eyes it is usually not seen; when the eyes are again opened it either has disappeared or is more or less faded. Such phenomena go to show that the phantasm is a mere illusion, constructed upon the sensory basis furnished by the medium. Sometimes, however, it seems to attain a degree of independence which would lend color to the notion that it has been subconsciously originated. I have sometimes found that, when once fully developed, it persisted when the eyes were closed, and even after the removal of the medium. With one subject it proved so durable that he was able to project it upon a sheet of white paper and trace its outlines. But voluntarily externalized images and after-images sometimes prove as permanent, and no one will claim for them a subconscious origin.

Like most hallucinations, the phantasm of the glass is quite independent of the idea-trains of the upper consciousness. Suggestion in the waking state seldom affects it, although it is readily amenable to hypnotic suggestion. In one case only have I found it capable of being affected by voluntary effort. One of my patients told me that while experimenting at home alone he heard the whistle of a locomotive. He forthwith fell to wishing that the locomotive would appear in the glass, and in a few moments it did. The smokestack, upper part of the boiler and a part of the tender appeared, but the picture remained incomplete, and by no amount of wishing could he force it to develop further.

Mr. F. W. H. Myers has called attention to the very interesting fact that if a story be related to a hypnotized patient, and it be suggested that he will see it in the glass, it will appear and be acted out in dramatic form. Many of my readers will remember that Mr. Myers gave illustrations of this before the International Congress in London three years ago. It occurred to me that it would be interesting to show that the glass would bring such a tale to light *without* any direct suggestion, and I tried to do it with a patient named Tom. He is a very ignorant man, but a good hypnotic subject. The experiment gave only negative results. He looked steadily for some time and saw nothing. I then asked, 'Don't you see so and so?' mentioning the first item of the scene he was to see. This slight suggestion proved sufficient and he saw the whole story acted out. I then hypnotized him again and said, "Tom, do you remember the old Greek story—how the giants piled one mountain on another and climbed up to heaven to pull the old god down from his throne?" Tom did not remember. I then bade him look in the glass, and he began to describe the event as he saw it, while I took his words down. He spoke slowly, with frequent pauses, and often moved his head or the medium about as if to get a better view. "It seems like there were some men in there—big giants or something—not very plain what they are—they've got little crowns on, olden style—got on long night-shirts, not very long, sort of cut short, you know, sort of lightish color—there are four

of them—they're moving—piling stuff up—can't tell what it is; guess it's rocks or stuff—whole lot of rocks; they're piling it up in the air—keep building and building up higher like—guess they're trying to reach as high as they can get. Pretty near as high as they can go now—they've got weapons with them, spears and such. Guess they're building a mountain on a mountain—away up now in the clouds. There's somebody else up there—some other man, I guess—looks as if they'd got hold of him and were pulling him off a throne or something. That's all I can see now." (I ask what the old man looks like,) "Dressed like wearing a pair of short pants with tights around his loins—breastplates on him, looks if he were a king or something. Don't see the fellow they pulled off—they are still there and have spears and weapons. Now they're gone too—pile of rocks is still there—now it's going too—it's all gone. Glass is clear." While telling this story the patient seemed to be in a hypnotic condition, as he commonly is while performing a posthypnotic suggestion, but was not suggestible after finishing, nor did he fall asleep as usual. If we could show that the glass tends to externalize suggestions given in hypnosis, it would go to show that there is some organic relation between the realms of consciousness laid bare in hypnosis and those reached by the glass. But at present I see no reason for assuming that there are any such realms existing permanently and subconsciously in all individuals. It is more congruent with the facts to suppose that we are dealing with more or less dissociated mental elements, between which there may be relation whatever, unless we 'set the switches' by suggestion and thus create one.

One would suppose, from *a priori* considerations, that the good visualizer would be more likely to see these images than others, and, so far as my facts go, this would seem to be the case. I tried 86 persons and got phantasms in 22 cases. Twenty of the 22 were young girls. and all were good visualizers. It would then appear that young girls who visualize well are the best 'seers.' But, on the other hand, 46 of my 86 patients were young girls under 22 years of age, pupils in a school where I was lecturing; and of the

remaining 40 not a half-dozen were above 30. Moreover, having early found an apparent relation between visualizing and 'crystal-vision,' I took pains to experiment with all the good visualizers I could find. Upon such selected material one can base no generalizations.

Before turning to other types of automatism, I will quote from my note-book two typical series of phantasms.

Miss E., age 20, well educated girl of a quiet and retiring disposition. Good visualizer. Has had visual hallucinations while in apparently good health. Begins looking in the glass at 9.13 A. M. [Figures indicate interval between entries.] In two minutes 50″, glass grows smoky; 2′ 40″, the smoke is forming into something; cannot make out what it is; 30″, the thing is moving around but does not settle into anything; 1′ 30″, it has become a little brownie, peaked cap and usual costume; is running; the legs move but the figure does not move from its place in the glass; 45″, it is gone; glass is still cloudy; 15″, looks very like the Coliseum; 20″, it is the Coliseum, but there is a little door below which she never noticed before; 10″, something else is coming; "What can it be?" she says; 'it looks like an animal and also like a human being;' 30″, it is a little old man with a great red nose. The Coliseum is still partly visible; the old man has long nose, sharp eyes, thin neck; 40″, the old man has disappeared, the glass is still smoky, the Coliseum still dimly visible in outline. What now, an alligator? No, not that; 10″, the old man back again; he looks younger now, but it is the same man; 30″, seems to be winking, his brows seem to go up and down; 15″, he seems to be turning around and looking the other way; 10″, he is gone; 5″, there he is again; 15″, he is still looking the other way. She has never seen him; 30″, he simply won't go away, she says; 10″, now he looks quite different, his nose is smaller and thinner, he has a helmet on his head, a determined looking man; looks like an ancient Roman; 1′ 20″, he is still there; 10″, I told her to shut her eyes; kept them closed five seconds; she saw nothing while they were closed, but the image was unchanged when she opened them again; 10″, he is gone at last; 35″, "There he comes again," said she;

"Oh, dear, dear, I wish he would go away;" 20″, he has turned into a bear; 10″, and now the bear has turned into a monkey; 10″, the image has entirely gone. All these images, she says, had the same eyes. Thirty seconds, "That dreadful looking man is coming back again"; 30″, no, it is another man, white robe, very large, smiling, costume like a monk's, not a Roman's; 30″, the head has faded out, the figure still there; when she moves her head it also seems to move. I put a black fountain pen between her eye and the object; it had no effect; 1′, all is gone except the milky or cloudy effect; 25″, a beautiful streak of blue appears; 10″, it is passing into a yellow; 10″, the colors are gone; 3″, now the color of heliotrope comes; 8″, it is becoming a rainbow, over the beauty of which she becomes enthusiastic; 24″, a window appears, a window-sill seat, interior view of a room, bookcases all around; 50″, it is gone; 20″, 'Oh, what is that trying to do?' 15″, 'I can't see what it is; the cloud moves into different shapes;' 45″, three dice appear; she sees the three and the five; cannot see the others. Stopped. Entire time, 23 minutes; no perceptible after-effect. Upon questioning her as to the source of the images, she said that she had been in Rome once, when about twelve years old. Had not been recently thinking or reading about kindred subjects. The old man, she thought, looked like one of Dickens' characters, but could not specify which one.

The only other case which I shall quote is that of Miss L., 18 years of age, an excitable girl, somewhat subject to slight hysterical attacks. Was in good health.

In three minutes 30″ sees a field, blue grass, stretching away off; gets pink by the horizon; there is a cloud on the grass, the cloud is getting pink; a face is coming out of the cloud; 1′ 25″, sees a figure to the face, gauzy drapery, pinkish near the cloud; cloud and drapery are the same; 35″, sky is blue; something or other is shining on the figure; cannot tell whether it is the sun or the moon; the hair of the figure is a bluish red, beautiful color; 30″, "The clouds are rolling, and the beautiful woman seems to be rolling on too; she is holding something which looks like two strings;" 7″, she thinks it is developing

into Guido's Aurora; 23″, sees one of the horses; there is only harness where the other horse should be; sees box of chariot but no wheels; 1′, there is something there which she thinks is a Cupid's head, but cannot see it clearly; 25″, the other horse is visible; the picture is complete except the 'Graces;' they are not there; the sun seems to be moving along; 50″, two or three Cupids appear; they are sitting on the chariot and elsewhere; 45″, the unfinished picture is turning very red, it is becoming a high brick wall; the luminous body, whether sun or moon, is still there, but the rest is gone except the wall; 20″, there seems to be a window in the wall; a beautiful girl is looking out; she has the same head and face that 'Aurora,' *i. e.*, Apollo, had; her hair is growing longer and longer; 1′ 5″, there seems to be a figure on the ground, it is a greyish-blue; 20″, seems to be playing on harp; sees only the head, arms and harp; the rest is cloudy; 25″, the clouds are becoming drapery; 10″, there are hills around and the sun is coming over the hills; 28″, the instrument seems to be something between a violin and a harp; 12″, the man has a red suit, a sixteenth century cape at back; 40″, half the field seems to be covered with her hair, which grows and grows; the man is wound up in it; 20″, the wall is gone; there seems to be only a sea of hair or water; 20″, the sun is still rising, it has been there all the time (probably a reflection); 40″, the hair is blue; it looks like the ocean; 30″, there seems to be something on the water; it looks like a shell; 30″, there seems to be a yellow head and a crown on the shell, eyes closed, the mouth opens; 45″, the hair comes again; it is red, short, crimped; the tongue sticks out; it is two yards long; 45″, there is a shore to the right; the tongue reaches to the shore, like a suspension bridge; the head slides along the tongue to the shore; 45″, the patient inadvertently took her eyes from the glass, and upon looking back the phantasm was entirely gone.

That auditory hallucinations in every way analogous to the phantasm of the glass may be produced by the application of a continuous but indeterminate stimulus to the organ is, of course, a familiar fact. Not long ago, for example, a

paranoiac was treated in the University Hospital who complained of a continuous hissing sound, which from time to time was transformed into abusive language. The noise was found to be due to a chronic inflammation of the inner ear· this had served as the sensory basis for the hallucination. A young woman, who has had several auditory hallucinations occurring apparently spontaneously, tells me that the sound of water running from a spigot always induces auditory hallucinations of precisely the same character as those above described in the case of vision, and they are often accompanied by appropriate visual pictures of the pseudo-hallucinatory order. Mr. Myers reports similar hallucinations produced by listening to the 'sound of the waves' in a large shell.

It is, however, in the phenomena of automatic writing that the most interesting illustrations of these principles are to be found. Automatic script is usually regarded as affording evidence of the existence of preformed, subconscious idea-systems which thus seek expression, and for a long time this was my own view. But I am now convinced that in many cases the writing is produced precisely as these sensory processes are produced—by the continuous application of an indeterminate stimulus to the highly organized writing mechanism. It is, then, essentially a purely motor phenomenon.

I was first led to this view by the study of a remarkably interesting case which was brought to my attention about a year ago. The patient, whom I shall call A. B., was an educated man, who had some knowledge of psychology and was acquainted with the conception of mental automatism. He was sitting one evening around a table with some friends, one of whom was supposed to be 'mediumistic,' to see whether rappings and levitation of the table could be had. Suddenly his left arm was drawn violently down; in a few moments the motor disturbance was transferred to the right arm. Pencil and paper were procured and the hand made desperate efforts to write. Much violence was displayed, the hand being pounded upon the table so furiously as to bruise the fingers and snap pencil after pencil. Nothing

legible was written. The case was brought to my notice in its early stages, and I had the opportunity of watching its later development. The violence at first displayed gradually disappeared. The hand learned to print and then to write a legible script, much resembling that of the patient. The content of the writing always professed to come from the patient's deceased friends—a claim which was readily disproved to the complete satisfaction of the patient himself. To him the hand seemed to be moved by some power not his own, yet he could at any time control it. The thoughts bore a marked resemblance to his own, but were not consciously furnished by him. He said it seemed to him as if he were watching another person write; often he would correctly anticipate the words to follow, but quite as often they would prove other than he had expected. At first B. himself believed that his dabbling in spiritism had started into activity a mass of subconscious idea-trains which had succeeded at the séance in bursting out into the motor mechanism. He was naturally not a little alarmed, and the further progress of his case did not tend to reassure him. The contracture of the right arm which always preceded the writing became easier to produce, and finally showed a tendency to appear spontaneously. At times he found it difficult to resist. Similar contractures appeared in the muscles of the left arm, of the legs, and finally of the face. By this time a general motor hysteria was produced. At the same time automatic sensory symptoms made their appearance in a manner that should delight the hearts of all who believe with Prof. Baldwin in the natural dependence of the sensory system upon the motor. Automatic idea-trains held converse with the patient at all times, seasonable and unseasonable; flashes of brilliant white light were occasionally seen while falling asleep, and the motor excitement rose to such a point that the patient escaped a hystero-epileptic convulsion only by two hours of strenuous resistance, followed by a timely dose of assafœtida. Throughout the patient believed himself to be struggling, like the possessed of old, with a secondary personality which was striving to overmaster his upper self.

This interpretation of the facts I believe to be entirely erroneous. They can be explained in a much more simple fashion. The original invasion might well have been a fatigue convulsion, due to the fact that the patient had for a long time held his hands outstretched upon the table. I have frequently seen contractures and convulsions produced under such circumstances, although not as violent as those of the patient are said to have been. The continuous but indeterminate stimulus applied to the centres—probably the subcortical centres of reinforcement and coördination—produce in time a reflex response. The neurologist who does it in order to study the phenomena gets purposeless contractions. The spiritist does it with the notion that the table is to shortly move under his hands, and the cortical processes which correspond to this expectation discharge downwards through the Rolandic region and impose upon the automatically produced contractions a semi-purposive character, producing lateral movements of the hands, which serve to move the table. I have seen this in more cases than one, and have proved to the satisfaction of the 'medium' that the movements of the table were due solely to the automatic contractions of his own arms. In the case of B., who had recently seen what purported to be spirit-writing, and had been much impressed thereby, the suggestion supplied by the cortical centres took a slightly different form and determined the otherwise meaningless movements to assume the form of writing. At first the patient allowed himself no expectation as to the content of the writing. But as he watched it scrawling away he naturally fell to wondering whether this was meant for such and such a word and that for another; he would ask whether it were, and then the word would be plainly written. Evidently, say I, in response to the suggestion given by him. As soon as he became accustomed to the thought that he might 'anticipate' the words his hand wrote, the process of genesis became easy. And it was made easier by a further suggestion which he gave himself. He asked the alleged spirit once when I was present by what means he communicated, and how it was that he, the patient, seemed to anticipate the spirit's thoughts. To which the writer replied that he supplied the thoughts himself tele-

pathically, it being the function of the 'medium' to write them down only! No wonder that the communications were thereafter much facilitated.

It was a comparatively easy matter to prove the true origin of these utterances as soon as our attention was drawn to it. We found that the hand never wrote anything intelligible when B. resolutely refused to attend to the writing. It was not necessary to look at it, as the motor sensation was sufficient to serve as a guide to what was being written, but it was necessary that B. should thus 'anticipate' it. If he did not the hand would write scrawls. He found also that the hand responded readily to all manner of suggestions. He had but to think, 'Why does it not try to print,' and forthwith it would print. He would ask the writer to bring a new spirit to write. It would flit through his mind that perhaps the new spirit would not be able to write. The new spirit would appear and write with labor and difficulty, or would print like a child. Then with 'practice' these characteristics would disappear and the newcomer would write as well as any *habitué* of B.'s organism. If while the hand was writing the illegible scrawls that always followed when B. did not help it out, he suggested, 'Why do you not try printing?' it would try printing, and produce page after page of curious symbols resembling some unknown language. I have no doubt that the 'unknown languages' often written by 'mediums' have a similar origin. On one occasion a 'new spirit' began writing a backhand. He announced that he was born in 1629, the figures having an archaic look. B. knew something of archaic script, and it occurred to him that the spirit wrote a hand suspiciously modern for one who was born in 1629 and died in 1685. Within a few lines the style changed to a twisted, gnarled hand, which certainly resembles a specimen of seventeenth century script with which I compared it. B. says that at the time of writing he thought the automatic script was written more rapidly than he could write archaic script of that character, but upon trying he found he could write identically the same hand voluntarily quite as readily.

Little by little the motor disturbance originally confined to the arm centres began, if my theory be correct, to involve

other centres. It first invaded the left arm centre, in response to the patient's suggestion, and then the legs and jaws. The development of the symptoms of ideal and sensory automatism I cannot analyze in detail. They were due doubtless to a progressive central disorganization of some kind, but I know no reason for it. Yet that the disturbance was primarily motor is proved by the fact that three or four days complete rest, with anti-spasmodic treatment, proved sufficient to put an end to it. B. still retained his power of producing the writing—a power which he rarely exercises, I believe—but the spontaneous symptoms entirely disappeared.

The teaching of such facts is plain. B. might well have become a classic case of 'secondary personality!' He presented many of the symptoms which are usually ascribed to subconscious idea trains, and I think I would myself, if he had not been an unusually good witness as to the subjective side of the phenomena, have put that construction upon them. As it is, this case, in conjunction with others of the same character, has thrown a flood of light upon the whole problem for me. I do not deny that mental states may exist subconsciously, and may be subconsciously integrated into complete dreams, and even into independent personalities. I would not wish to question, upon the strength of the four or five cases that I have studied, the conclusions of such careful investigators as Pierre Janet and Binet. Yet it is very difficult to correctly interpret the significance of these automatic phenomena in terms of consciousness when the patient is incapable of giving any clear account of their subjective feeling. We must remember that, while consciousness is revealed to us through the motor mechanism only, it does not necessarily follow that a given motor phenomenon always bears witness to the existence of the conscious state which it usually expresses. In these cases the significance of the motor phenomena was undoubtedly due to states in the upper consciousness, to which they at first glance seemed to bear no relation whatever, but it seems quite possible that apparently significant motor phenomena of a high degree of complexity may be produced by the subcortical mechanism without the coöperation of the cortex, and in all probability without any form of accompanying consciousness.

WELLESLEY COLLEGE PSYCHOLOGICAL STUDIES.

DIRECTED BY MARY WHITON CALKINS.

I. Dr. Jastrow on Community of Ideas of Men and Women.

BY CORDELIA C. NEVERS.

In an article called 'A Study in Mental Statistics,' which appeared in the December, 1891, number of the *New Review*, Prof. Joseph Jastrow discusses, among other subjects, "The Community of Ideas and Thought-Habits of Men and Women." His data are fifty lists of one hundred disconnected words, twenty-five from the men and twenty-five from the women of his Wisconsin University psychology class. His chief conclusions are "that women repeat one another's words more than men," and that "there is less variety among women than among men;" that the feminine traits revealed are "an attention to the immediate surroundings, to the finished product, to the ornamental, the individual, the concrete; while the masculine preference is for the more remote, the constructive, the useful, the general, the abstract."

For the purpose of testing these results the young women of the experimental psychology class at Wellesley College were asked to write out similar lists of one hundred disconnected words. That the thought-process might be as free as possible, no restriction was made. The students were not even asked, as in the case of Dr. Jastrow's class, to write as rapidly as possible, but this difference in the method cannot possibly be supposed to account for the wide difference in results. The subjects had no knowledge whatever of the purpose of the investigation. Twenty-five papers from those first completed were selected for the comparison; all were

rejected which were not declared by the writers to be the result of a natural flow of thought, and two were set aside because the words formed sentences.

According to Dr. Jastrow's first generalization, 'women repeat one another's words more than men.' He finds that "female students in their 2,500 words used only 1,123 different words, while their classmates used 1,375. Again with regard to classes, the women show different preferences from the men. The repetitions in the names of articles of apparel, of interior furnishings, predominate with women. Similarly the men have more repetitions in the names of animals, and mention more such names." The Wellesley lists tell a different story. Only fifteen lists, that is 1,500 words, have been studied from this point of view, but among the 1,500 are 1,103 different words, only 20 less than those among the 2,500 of the Wisconsin University women, and only 272 less than those among the 2,500 of the Wisconsin University men.

Further comparison is based upon Dr. Jastrow's division of his words into twenty-five classes. There may, of course, occur a deviation from Dr. Jastrow's principles of assignment of particular words to appropriate classes, but with reference to the classes which he especially emphasizes (words of wearing apparel, for instance, and abstract terms) there is no chance for difference of opinion. In the following tables of results Dr. Jastrow's order of frequency has been followed:

Comparative table of frequency of mention:

	Men of Wis. Uni.	Women of Wis. Uni.	Women of Wellesley.
1. Animal kingdom,	254	178	223
2. Wearing apparel and fabrics,	129	224	96
3. Proper names,	194	153	141
4. Verbs,	197	134	114
5. Implements and utensils,	169	121	132
6. Interior furnishings,	89	190	84
7. Adjectives,	177	102	234
8. Foods,	53	179	56

9. Vegetable kingdom,	121	110	91
10. Abstract terms,	131	97	280
11. Buildings and building materials,	105	117	106
12. Parts of body,	101	105	34
13. Miscellaneous,	91	99	162
14. Geographical words,	97	80	142
15. Mineral kingdom,	74	96	54
16. Meteorological and astronomical,	85	76	26
17. Stationery,	60	89	26
18. Occupations and callings,	71	47	33
19. Conveyances,	62	52	79
20. Educational,	34	76	167
21. Other parts of speech,	96	5	41
22. Arts,	33	61	44
23. Amusements,	30	53	102
24. Mercantile terms,	30	29	15
25. Kinship,	117	32	18
	2500	2500	2500

The differences between these Wisconsin and Wellesley women's lists are very striking. According to Dr. Jastrow's results, the class to which women contribute most largely is that of 'articles of dress.' Of every 11 words, 1 belongs to this class, and the women have 224 such words as 1 against 129 on the men's list. Our records, on the other hand, swell the numbers of this class to only 96 words, giving it but 1 word in every 26. And if arranged in the Wellesley order of frequency this class would have been twelfth instead oi first.

In the sixth class, again, our results differ widely from those of Dr. Jastrow. He writes: "The prevalence of words denoting the common articles of furniture and interior fittings of a house—the peculiar field of woman's household instincts—is quite as marked, such objects being mentioned 190 times by women and 89 times by men." Our lists include only 84 such words, suggesting, perhaps, a lack oi household instinct on the part of the Wellesley students, who appear less domestic than even the Wisconsin University men. In the class of 'foods' the disproportion of the Wisconsin record is even greater: the women mention words

of this class 179 times and the men only 53 times. The Wellesley records include, however, only 56 words of this sort.

In reference to words referring to amusements, arts and educational matters, our statistics corroborate the statement of Dr. Jastrow, that, in these subjects 'women show an excess over the men. "This," he observes, "points to a characteristic difference in the interests of the two sexes." The Wellesley students mention words referring to amusements with suggestive frequency—102 times.

"In the young men," Dr. Jastrow says, "we see marked preference for names of animals, of implements and utensils, the names of professions and similar relations." Up to this point our results agree, though the distinction which he notes is not in every case so marked. But the preponderance of abstract terms in the men's lists, which Dr. Jastrow accentuates strongly, vanishes utterly when these records are compared with those of the Wellesley students. Our lists contain 280 abstract terms, not only more than those (131) of Dr. Jastrow's men, but more than those (228) of all fifty of his students. In one paper alone occur fifty abstract words; the writer was specially questioned, and insisted that the progress of the thought had been entirely natural. If this unusual record be omitted, there still remains a number slightly greater than the Wisconsin total. It should be added that the Wellesley students earlier in the year had written several associated lists, involving almost necessarily the use of concrete terms, so that any artificial inclination to abstractness was unlikely. The prevalence both of abstract terms and of adjectives in the Wellesley lists is of questionable rhetorical import.

The conclusions from this study are thus chiefly negative, but not for that reason unimportant. The figures are too few to allow any positive deductions, but their uncompromising contradiction of Dr. Jastrow's results gives a needed warning concerning the dangers of such comparative study of the mental processes of men and women. If the serious study of the supposed psychic differences between the sexes can lead to conclusions so opposite to each other, the worth-

lessness of ordinary generalizations is made very clear—based as they are on purely personal observation and, usually, on the comparison of men and women of entirely different training.

II. PREVALENCE OF PARAMNESIA.

BY MARGARET B. SIMMONS.

An incidental corroboration of the ordinary opinion that paramnesia, in its various forms, is very common among normal people, was obtained in the course of some experiments on association[1] with the Wellesley subjects already mentioned, and with members of the Harvard Psychological Laboratory. A series of colors, each of them quickly followed by a numeral, was shown; and afterwards the colors alone were exposed, in succession, with the request that whatever numeral occurred to the mind of the subject, at the appearance of each color, should be written down. The main object of the experiment was to discover the number of times in which that numeral which had appeared in the first series in close connection with a color, would now be associated with the same color appearing alone. The subject was also asked, however, immediately after writing down a numeral, to record by the use of the signs +, — and ?, his opinion of the correctness of the association. These opinions form the material of the following summary of cases of a form of paramnesia, which may be called False Recognition.

Regarding first the totals, it will be observed that almost half (47%) of our entire number of recorded opinions are instances of paramnesia. The first vertical column of the table contains, as will be observed, the number of cases of what may be called negative paramnesia, not the persuasion that a mere object of imagination is a memory image, but the failure to recognize an actual reproduction. This distinction may best be studied by reference to the analysis of the phenomena of memory into two factors (objective and subjective), namely, recurrence or reproduction, and recog-

[1] *Cf.* PSYCHOLOGICAL REVIEW, Sept., 1894.

nition. These cases of 'negative paramnesia,' in which the correct numeral is judged to be wrong, involve actual recurrence, unaccompanied, however, by recognition; the instanstances in which the incorrect numeral is judged to be correct involve a failure of reproduction, as well as a lack of recognition. The fact that this latter class of pseudo-reminiscence contains 323 cases, or three times as many as the class of negative paramnesia, indicate that these factors, recurrence and recognition, strengthen each other or fail together.

FALSE RECOGNITION.

	The numeral written *actually is:*			Total.
	Correct.	Incorrect.	Half correct.	
Correct recognition, ...	*372* (76.54%)[1]	*104* (24.35%)	*75* (59.05%)	*551* (52.98%)
False recognition (paramnesia):				
Numeral, *judged* correct, ...	0	90	29	119
Numeral, *judged* incorrect, ...	36	0	23	59
Numeral, *judged* doubtful, . . .	78	233	0	311
Total paramnesia,	*114* (23.45%)	*323* (75.64%)	*52* (40.94%)	*489* (47.01%)
Total cases .	486	427	127	1,040

[1] The per cents are calculated on the totals at the foot of the vertical columns.

SENSORY STIMULATION BY ATTENTION.

BY PROFESSOR JOHN GRIER HIBBEN,

Princeton University.

The important function which is attributed to attention in the processes of sense-perception is very strikingly illustrated in an interesting instance which has recently been brought to my notice, and which throws into sharp relief the phenomena of attention operating in an intensified manner, and consequently modifying sensation to an extreme extent. It very often happens that normal tendencies are most clearly exhibited in unusual or abnormal cases, because there then occurs an inhibition of counter or complimentary tendencies, thus presenting to the observer the unique operation of an undisturbed and unmodified function. Instead of the resultant of many, there is the sole functioning of the one element separated from its usual concomitants. A force thus revealed in isolation is more readily appreciated whenever seen conjoined with accompanying forces in any system however complex.

The instance I wish to present seems to me to be of this nature, a case where the normal functioning of the attention is intensified in a very unusual degree. It is the case of a child about eight years old, a little girl, who as a baby was supposed to be congenitally deaf, as she gave no evidence of hearing any sounds whatsoever. Somewhat later in her development, however, it was noticed that at certain times she seemed to hear, and this always in connection with some objects of special interest, as bright pictures, toys, etc. And this now characterizes her general ability to hear: whenever the subject is one especially interesting to her, she hears without great difficulty; but whenever there is no interest in conversation, it is with greatest difficulty that she can be made to hear at all; and it is impossible to gain her

attention by any sounds, however loud, if she is engrossed in any absorbing task or play. Connected with this naturally there was an extremely tardy beginning of speech and very slow development, though during the last year—her eighth year—there has been a marked acceleration of her progress in this particular. In Preyer's classification of the imperfections and derangements of speech there is no precise place for such a case as this, as it is neither peripheral deafness, nor yet does it seem to be any cerebral derangement.[1] The difficulty seems to be psychical rather than physiological.

The facts as I have given them were narrated to me by a trained nurse, graduate of the New York Hospital Training School, who was in my family for a month or more during this last winter. She had been for several months in the family attending the mother of this child, and had had abundant opportunity for observation and for acquiring accurate information upon the subject. Moreover, she is a woman of very unusual ability, as one of the visiting surgeons of the New York Hospital assured me, and therefore one whose account can be wholly relied upon. In addition to her report of the case, however, I called upon a physician in New York who had known and observed this child for several years, and he fully corroborated the statement as made by the nurse in all particulars. I learned from him also that the child had been examined three different times by an eminent specialist in New York, and no defects either in the outer or middle ear could be detected. The indications all pointed to normal peripheral conditions, and the marked variations in hearing seem due to the central fluctuations of interest and the corresponding concentration or distraction of the attention. This appears only as an exaggerated form of the normal affect of interest upon attention. As Bradley[2] says: "If an idea engrosses, then any sensation which is connected with that idea may in consequence engross. And attention so far has appeared to consist in interest, either direct or transferred; an account which, we shall find, will

[1] *The Development of the Intellect*, p. 36 ff.
[2] *Mind*, Vol. XI, p. 310.

hold good everywhere" (*Cf.* Waitz, *Lehrbuch*, 634–7). In the ordinary phenomena of hearing, we recognize two momenta, the external stimulus and the inner adaptation of attention. According to Prof. James,[1] "the natural way of conceiving this is under the symbolic form of a brain-cell played upon from two directions. Whilst the object excites it from without, other brain-cells, or perhaps spiritual forces, arouse it from within. The latter influence is the 'adaptation of the attention.' The plenary energy of the brain-cell demands the coöperation of both factors. Not when merely present, but when both present and attended to, is the object fully perceived." Now, in sense-perception the two momenta, stimulus without and attention within, are normally so related that the former generally predominates, and is capable of arousing the activity of the latter, which is thus in a sense a function of the former, always reacting when the stimulus is sufficiently intense. In the case which we are considering, however, the attention does not function in an instinctive manner in response to an outer stimulus, and seems independent of its degree of intensity; but is readily aroused by the inner interest, and then alone is the consciousness of the outer stimulus rendered possible. This child, for instance, understands the sign language, and that is resorted to in order to communicate with her until interest has quickened the attention, and that in turn has stimulated the hearing. This is similar, in a much lessened degree, to the ordinary cases of what Herbart styles apperceptive attention—viz., where strained attention brings to consciousness external stimuli otherwise unnoticed. And this is similar also to what Prof. James[2] refers to as the 'ideational preparation' for sensation, in which, of course, there is increased attention, reinforced by the dominant idea present in the mind. The function of attention in sense-perception is illustrated by Wundt[3] with weak auditory stimuli, as the ticking of a watch at some distance from the ear, so that it can be perceived only with some strain of attention,

[1] James, *Psychology*, I, p. 441.
[2] *Psychology*, Vol. I, pp. 453, 439.
[3] Wundt, *Human and Animal Psychology*, pp. 256–7.

but will fall below the limen of consciousness without any relaxation. At intervals of three or four seconds the regularly recurring impressions alternately appear and disappear. In this child's case, however, the attention is not merely a factor necessary to discriminate concerning very weak stimuli, but the very strongest stimuli cannot excite the attention through reaction; it can only be centrally aroused; the hearing, then, being a function of the attention in her case, rather than the two being complementary functions determined by a law of action and reaction.

Moreover, a child's attention is characterized ordinarily by an extreme susceptibility to the sights and sounds of the outer world, and responds almost instinctively to sensorial stimuli. Sustained concentration of attention in childhood is unusual.[1] It is in mature age that attention follows our permanent interests, and only those objects associated with such interests find place in consciousness. Absorption in contemplation occurs only when a large group of associations have for years been forming about the controlling interest. And even then, with interests of greatest compelling power, diversion occurs whenever sensorial stimuli are sufficiently increased in intensity. This child, however, can have no developed associations of any considerable extent around her controlling interests; and yet her absorption in the same is complete, and her attention is incapable of being distracted. There is also a marked difference in her increased ability to hear whenever questioned concerning scenes which she has herself witnessed and in which she has taken part with evident pleasure to herself. For instance, after attending an exhibition of Hagenbeck's animals in New York, she heard and replied to all questions put to her concerning the animals and their performances. In this no doubt there was an ideational reinforcement of the auditory stimuli through the memory pictures still vividly impressed upon her mind. The ideational processes causing motor discharges which in time would increase the intensity of the sensation. This would form a stimulus additional to the mental energy arising from the increased interest already

[1] James, *Psychology*, I, p. 417.

mentioned.[1] In this connection it may be of interest to quote a sentence from Prof. James that bears upon this point: "We see how we can attend to a companion's voice in the midst of noises which pass unnoticed, though objectively much louder than the words we hear. Each word is *doubly* awakened; once from without by the lips of the talker, but already before that from within by the premonitory processes irradiating from the previous words, and by the dim arousal of all processes that are connected with the 'topic' of the talk. The irrelevant noises, on the other hand, are awakened only once. They form an unconnected train."[2]

In accounting for such a phenomenon, it is well also to take into consideration the physiological conditions which tend to increase the intensity of a sensation when the attention is unusually concentrated upon it. There seem to be indications of increased circulation in the parts concerned. This is stated by Dr. Cappie in his article on 'The Physiology of Attention and Volition'[3]: "The mental effect produced by an impression on a sensory surface is stronger, and details about the impressing cause are more completely gathered in when the mind is concentrated on it. . . . Two factors, at least, may be specified as bearing on this problem. In the first place, when the consciousness is engrossed by an immediate sensation, the sphere of encephalic activity is comparatively restricted. In the second place, the encephalic circulation will be focused in the direction of activity. The molecular agitation occasions a necessity and an attraction for more blood, and determination of this takes place all the more freely on account of the quiescence of the large part of the brain. The latter has, as it were, loosened its hold on the circulation, and the impetus towards those parts which have an attraction for it is thus all the stronger. The increased activity of the circulation then reacts on the energies of the tissue, and the mental effect produced is therefore greater."

[1] Baldwin, *Mental Development*, pp. 462–3.
[2] James, *Psychology*, Vol. I, p. 450.
[3] *Popular Science Monthly*, Vol. XXX, pp. 231–2.

Now, in the case of this child, it seems as though the conditions, both physiological and psychological, are present in exaggerated form, in order to produce the unusual results. The difference, however, between this case and normal instances is a difference rather of degree than of kind.

An additional feature of interest lies in the similarity between the phenomena we have been considering and the phenomena often accompanying attacks of hysteria. In such cases there is generally a very restricted field of attention, and the patient becomes so completely absorbed in some engrossing subject as to appear completely oblivious to all sensorial stimuli whatsoever. For instance, take such a case as cited by Pierre Janet: Lucie, while talking to one person, was insensible to all sounds about her, and could even be touched without being conscious of it. And when Léonie was knitting or writing, you might open or shut the door with a loud noise, speak to her, touch her, etc., without her perceiving it at all. Moreover, there were parts of her body which were so extremely sensitive to touch as to provoke cries of pain and even convulsions; and yet, when preoccupied by work or simple conversation, she could be touched upon the same hyperæsthesia spots, with no indication that she perceived this.[1] This account is very similar to the report which I received in reply to a letter which I wrote to the nurse above mentioned, making further inquiry concerning this child's case, and asking particularly whether she could hear when spoken to from behind, where her attention could not be aroused by any visual stimulus. The following is the answer which I received: "Her ability to hear when interested, in comparison with times when not interested, is very marked. She can hear if you stand behind her and talk very loud, but not very well; and *never* when she has her mind concentrated on another object; for instance, if she is at a window, looking out at something which has her attention, it is impossible to make her hear."

A case somewhat similar, yet with certain interesting peculiarities, is quoted by Pick from Pitres'[2] *Leçons cliniques*

[1] Pierre Janet, *L'Automatisme Psychologique*, pp. 188–9.
[2] *Zeit. für Psych.*, p. 168.

sur l'hysterie: "The patient, with eyes open, could hear; but with eyes closed could not; with one eye open it was possible to hear in the ear opposite, but not in the ear upon the same side as the opened eye." In this same article by Pick, *Ueber die sogenannte Conscience Musculaire (Duchenne)*, there is a general historical survey from the time of Duchenne to the present concerning cases of anæsthesia, in which motor activities have been mediated through visual attention, indicating the quickening of motor as well as sensory functions, by means of a concentration of attention. In all cases where there was not this aid of attention through auditory or visual direction, the attempted movements were impossible. It has been also observed that in the somnambulistic state subjects are sensitive to the voice of the hypnotizer, but do not hear other voices. M. Janet mentions the case of one in this condition who could see a candle lighted by himself, but not those lighted by others; and adds that such "is not peculiar to the somnambulistic state, but exists in high degree among all persons susceptible to suggestion. It is an exaggerated state of distraction which is not merely temporary, and not the result of voluntary attention directed exclusively to one sense; but it is a state of natural and perpetual distraction which prevents these persons from perceiving any other sensation than that which actually occupies the whole field of consciousness."[1]

The case of this child seems to occupy a position midway between the temporary and permanent states of distraction as mentioned in M. Janet's account. In all of the similar instances which I have given, as parallel to this case, I have endeavored to indicate varying stages of concentrated attention from the normal to the exaggerated and abnormal; and as closely related phenomena we may consider them as different manifestations of one and the same tendency—the intensified force of attention producing an exaggerated modification of the intensity of sensorial stimuli.

[1] P. Janet, *L'Automatisme Psychologique*, p. 189.

SHORTER CONTRIBUTIONS.

PRACTICAL COMPUTATION OF THE MEDIAN.

BY E. W. SCRIPTURE,

Yale University.

In the PSYCHOLOGICAL REVIEW for Jan., 1894, I called attention to the practical and theoretical advantage of the median over the arithmetic mean. Since that time the median has been employed in many thousand adjustments with entire success. The theoretical justification has, however, been lacking, and correct formulas have not been given.

In the *Studies from the Yale Psychological Laboratory*, 1894, II, 5, I have presented a theoretical consideration of the median. Owing to condensation the practical computation has not been explained in such a way as to be available to the non-mathematical reader. Fechner is the only one who has attempted to give practical rules. They are not quite correct, and, as experience during the past year has shown, are incomplete. I propose here to put the matter in a practical shape for actual computation, all theoretical discussion being avoided. Suppose that you have a set of n measurements, $x_1, x_2, \ldots, x_n$. Starting with the smallest value, check them off in order of size till you come to the $\frac{n+1}{2}$th value. Then, as a verification, start with the largest value and check off downward to the $\frac{n+1}{2}$th value. For an odd number of measurements the two results will agree; for an even number they will agree or be adjacent in order of size.

For example, suppose we have the following results of a set of experiments on reaction-time: 213, 215, 214, 210, 212, 214, 215, 210, 212. There are 9 values, and the middle value is the $\left(\frac{9+1}{2}\right)$th or 5th. The smallest is 210, the next smallest is 210, the next 211, etc. The 5th is 213. The largest is 215, the next 215, etc. The 5th is 213.

Let us take, as another example, the set of values: 44, 51, 46, 50, 47, 49, 47, 45, 48, 50. The median will be the $\left(\frac{10+1}{2}\right)$th or $5\frac{1}{2}$th value. The 5th from the smallest is 47; the 5th from the largest is 48; the $5\frac{1}{2}$th will naturally lie between the two, and we take $47\frac{1}{2}$ as the median.

A third variety of examples arises by taking a set of results where several values fall on the same number as median. Suppose we have 127, 123, 121, 123, 125, 124, 121, 120, 122, 123, 124, 123, 123, 123, 123, 122, 121, 125, 124, 121, 122.

Result, ...	120	121	122	123	124	125
Times of occurrence,	1	4	3	5	4	2

The $\left(\frac{19+1}{2}\right)$th or 10th value is 123. The expression $M = 113$, however, does not mean $M = 123{,}000\ldots$ but $123.5 > M > 122.5$, because we have been rounding off by errors of scale and errors of number all records to the unit-place instead of writing them to an indefinite number of decimals. Owing to the fact that 5 values fall upon 123, we know something about the first decimal place.

We are entitled to suppose that all values of 123 have arisen from values evenly distributed between 122.5 and 123.5. Counting from the smallest end, we have 8 values up to the limit of the region covered by the middle values; counting from the larger end, we have 6 values down to the other limit of this region.

The general formula is expressed as follows: Let r be the value on which the median falls, and let there be m such values. Let the number of values above r be a and below r be b. Then take $c = a - b$ and

$$M = r + \frac{c}{2m}.$$

This will give the decimal places for the median.

In the last example $b = 8$, $a = 6$, $c = -2$, $m = 3$, $r = 5$, and consequently

$$M = 3 + \frac{-2}{10} = 2.8.$$

Although we know something about the first decimal place, we know nothing of any worth regarding the second decimal place unless $m > 100$.

To make clearer the third kind of example, I subjoin a set of specimens that show how the changes in c affect the value of M.

1.)

$$\underbrace{11\ \ 12}_{b=2} \qquad \overset{r=13}{\underbrace{13\ \ 13\ \ 13\ \ 13}_{m=4}} \qquad \underbrace{14\ \ 15\ \ 15\ \ 15}_{a=4}$$

$$c=2 \qquad M=13+\tfrac{2}{8}=13\tfrac{1}{4}$$

2.)

$$\underbrace{11\ \ 12}_{b=2} \qquad \overset{r=13}{\underbrace{13\ \ 13\ \ 13\ \ 13}_{m=4}} \qquad \underbrace{14\ \ 15}_{a=2}$$

$$c=0 \qquad M=13+0=13$$

3.)

$$\underbrace{10\ \ 11\ \ 12}_{b=3} \qquad \overset{r=13}{\underbrace{13\ \ 13\ \ 13\ \ 13}_{m=4}} \qquad \underbrace{14\ \ 15}_{a=2}$$

$$c=-1 \qquad M=13-\tfrac{1}{8}=12\tfrac{7}{8}$$

4.)

$$\underbrace{9\ \ 10\ \ 11\ \ 12}_{b=4} \qquad \overset{r=13}{\underbrace{13\ \ 13\ \ 13\ \ 13}_{m=4}} \qquad \underbrace{14}_{a=1}$$

$$c=-3 \qquad M=13-\tfrac{3}{8}=12\tfrac{5}{8}$$

5.)

$$\underbrace{9\ \ 10\ \ 11\ \ 12}_{b=4} \qquad \overset{r=13}{\underbrace{13\ \ 13\ \ 13\ \ 13}_{m=4}}$$

$$c=-4 \qquad M=13-\tfrac{4}{8}=12\tfrac{1}{2},$$

6.)

$$\underbrace{9\ \ 10\ \ 11\ \ 12}_{b=4} \qquad \underbrace{13}_{m=1} \qquad \underbrace{14\ \ 15\ \ 15\ \ 15}_{a=4}$$

$$c=0 \qquad M=13$$

The computation in the last two kinds of examples is so

simple that it can be performed with great rapidity. Those of the other kind are almost as easy as soon as the computer becomes familiar with the median. Indeed, the economy of the median, in respect to time is something that can be appreciated only by some one who is obliged to compute many sets of results.

THE SECOND YEAR AT THE YALE LABORATORY.

BY E. W. SCRIPTURE,

Yale University.

Probably the most important event during the year was the introduction of the median as a mean value in place of the arithmetical mean. Early in the year it became evident that the arithmetical mean was a poor one to use for the results obtained and the median was substituted. This value is the middle one from either extreme in a set of values arranged in the order of size. It had been theoretically discussed by Laplace and Fechner, but had never been put to practical use. The main reason for this radical departure can be summed up briefly as follows: 1. Psychological and statistical measurements almost always follow an assymmetrical law of probability, and for such cases the median is a better representative than the arithmetical mean; 2. the saving in labor of compensation is very great; 3. the small numerical disadvantage is almost never of importance. The first article of the second issue of the *Studies from the Yale Psychological Laboratory* makes an attempt at a thorough theoretical treatment of the median and a comparison of it with the arithmetical mean. The article, although crude and incomplete, may derive some value from the fact that the author subjected it to criticism and correction by several mathematicians, notably Prof. Gibbs.

The most extensive investigation of the year was by Mr. Gilbert on the mental and physical development of school children. About 1,400 children were subjected to tests and measurements on the muscle-sense, color-sensitiveness, suggestibility, reaction-time, discrimination-time, time-memory,

voluntary motor ability and fatigue, weight, height and lung-capacity. The ages are from 6 to 17. Tables and curves of results show the laws of development with age.

The work is presented in the thesis in so condensed a form that it is impossible to further summarize it. Its great value probably lies in the opening-up for investigation the field of mental life in school-children by the invention of suitable methods and apparatus. It is a contribution to child-study and anthropology, in which the methods of the new psychology are applied for the first time. The use of mean variation, of two kinds for the study of the children is a notable innovation.

An investigation on the highest audible tone led to the following conclusions: 1. The pitch of the highest audible tone varies directly and almost proportionately with the intensity; 2. the highest audible tone is much higher when proceeding from silence to sound than when proceeding from sound to silence; 3. fatigue has apparently no effect; 4. fluctuations were found similar to the fluctuations for the threshold of intensity; 5. above the highest audible tone there is still found an indefinite, somewhat painful sensation.

Experiments on the education of muscular control and power lead to the conclusions: 1. Steadiness of movement can be increased by practice; 2. this increase in steadiness is not limited to the muscles immediately trained, it affects the control of the corresponding muscles on the opposite side of the body; 3. the training consists principally in training in steadiness of attention; 4. the increase in muscular power by training, as measured by a dynamometer, is partly transferred to the opposite side without training.

Some experiments made on fencers and others seem to justify the statements:

1. The average fencer is not quicker in simple reaction (where a few mental elements are involved) than a trained scientist.

2. When once the mind is made up to execute a movement, fencers are far quicker in the actual execution.

3. As the mental process becomes more complicated, the

time required by the average fencer is greater than that required by a trained scientist.

4. The general conclusion seems to be that fencing does not develop mental quickness more than scientific pursuits, but it does develop to a high degree the rapidity of executing movements.

The investigations mentioned thus far are published in the *Yale Studies*.

Throughout the year the investigation of hallucination and suggestion has been continued; it will be brought to a close in the coming year.

An investigation on the least perceptible change in the intensity of light has been carried on at intervals during two years, but is still left unfinished.

The scientific policy of the laboratory remains unchanged, the main effort being to train the members to accurate and reliable work. That this is not always successful is generally due to previous defective mathematical training.

In the account of the laboratory for the first year (PSYCHOLOGICAL REVIEW for Jan., 1894), mention was made of the success of the small workshop. During the past year the equipment was largely increased by the introduction of a two horse-power motor, shafting, etc., and by the purchase of new tools. It is the desire of the laboratory to offer facilities to all psychological institutions for the construction of fine apparatus.

The officers of the laboratory during the year were: E. W. Scripture, instructor and director, and C. B. Bliss, lecturer and assistant. Mr. J. J. Hogan was employed as instrument maker and electrician. Dr. Bliss left at the end of the year to take the place of assistant professor in the University of the City of New York.

DISCUSSION.

THE NEW PSYCHOLOGY IN UNDERGRADUATE WORK.

The rapid development of laboratories for the study of Physiological and Experimental Psychology has silenced the chief objections to such methods of investigating the facts of mind. It is doubtful if any other subject ever experienced as rapid changes in method and point of view as has psychology during the last five years. We need not go back fifteen years to find the beginning of the first laboratory in America. The first was also the only one till 1887. During the next three years about one laboratory per year was begun in this country; then several came at once, and for three years thereafter they fairly poured upon us. The past year has witnessed the establishment of fewer well-equipped laboratories, probably because all the more important schools of our land had already inaugurated the work. The universities of Michigan, California and Minnesota, in which psychology has always been subordinated to History of Philosophy and Ethics, have not attempted to withstand the tide. For the past two years the chief progress, perhaps, has been in the normal schools and in modifying the methods of teachers of philosophy in the smaller colleges. The text-books of men who are believed fairly to represent the attitude of Harvard, Yale and Princeton toward the 'materialism' of the new movement, have done more than all else to make easy the growth of modern psychology in our denominational colleges and in our normal schools.

Now that a firm foothold is acquired, may we not profitably look about us to discover, if possible, where we stand? The establishment of more than thirty psychological laboratories in this country in the last five years was not the result of a carefully matured plan to revolutionize instruction in philosophy. Indeed there has been no planning done for the college world at large, and unfortunately, perhaps, very little for any institution in particular. The men and women at the head of these laboratories have come from several independent schools for original investigation, or from one or two schools in this country with much the same object, or have picked up their knowledge by reading and private experimenting. Probably not one of these directors has followed any general plan in use

elsewhere. We have, therefore, as many ideals as we have individual directors. Let nothing be said unfavorable to such a condition. It has doubtless made stronger men and has been a prominent factor in the extraordinary success of the new movement. Even we, however, may learn from the experience of others, and our successors must depend largely upon our work.

It does not seem out of place to inquire into some of these ideals with a view to determine the nature of their agreements and the extent of their differences. If sufficient interest in this subject exist, a discussion of the aims and possibilities of the new psychology may lead to a considerable extension of its usefulness without necessarily interfering with the individuality of the various laboratories and their already efficient directors. It is with a hope to provoke such a discussion that this short paper is written.

In the first place, we note that the new psychology has not taken the place of the old psychology, in the sense of having been substituted for the latter in the required course of study. While nearly all colleges formerly required psychology for graduation, at least in some of the courses, there are very few (I know of none) that require physiological and experimental psychology for graduation in any course. The extension of the elective system has been a most potent means of introducing the new work. In what different ways this is true need not be enumerated, as nearly every school has made the change within the last few years.

Most of the older universities were content to require a certain amount of 'philosophy,' usually a smaller amount, than formerly; the student was thus at liberty to select his specific work with the consent of the professor in charge. In many of the newer universities, especially in the Central States, philosophy was eliminated entirely from the required work in some or all of the numerous courses leading to the bachelor's degree.

We may now see some effects of these steps when taken in conjunction with the following facts: First, in the larger and older schools several professors are engaged in teaching the different subjects generically called philosophy. As a rule these men are scholars of experience, power, skill, influence, and reputation. Also, as a rule, the new instructor in experimental psychology has been a recent graduate lacking some of the above-mentioned qualities. It is very evident why it has seemed inadvisable to attempt to develop an undergraduate course that would attract the general student. Hence, in nearly all large universities of the East, experimental psychology is considered a post-graduate, or at least an advanced,

philosophical discipline. Students are admitted to its work under various conditions, but generally after work in other lines of philosophy. If we all feel now as I felt when I first determined to study the new psychology, this plan would be quite rational—indeed conscience would demand it; the intellectual, moral and religious safety of the students would require such a preparation against the evil temptations therein. There is, of course, no question of conscience in this method. It is merely the result of conditions that are difficult to remove, and perhaps, in most cases, whose removal is not desired.

The additions to this young science must come mostly from men whose professional duties are light and whose energy is commensurate with their leisure. These professors and their well-trained students furnish valuable contributions every year. The animating spirit, the successful progress and the new recruits must always come from men whose entire time is given to research or to students engaged in research. The post-graduate work in psychology is even more necessary than such work in other departments. The following lines, therefore, will not be understood as an attack upon post-graduate work in psychology, nor even as an underestimation of the value of such work.

Is there a field for experimental psychology in undergraduate work? The writer knows there is sometimes in some places, and will endeavor to present a few points that seem to him to indicate a wider field for such work than, according to his personal knowledge, is now covered.

After the foregoing assertions it will not be surprising to learn that the facts in support of my present thesis are furnished mainly by a few Western universities where laboratories for undergraduates have been successfully conducted with classes, largely elective, whose numbers form as large a proportion of the entire student body as do the classes in any branch of philosophy in other institutions. It does not follow, however, that the conditions of success in these lines are peculiar to the West, though it is probably owing to the greater relative importance attaching to the instructor or professor of the new psychology. In small schools there is seldom another with whom to share responsibility or authority. The sole professor of philosophy may conduct his work as the chief executive and the faculty will endure. These external conditions are more favorable in the small school. I shall not attempt to show that in large schools the corresponding less favorable conditions can be improved. It is the internal evidence that must now engage our attention.

It is not sufficient to show that large classes in physiological and experimental psychology are organized every year in several Western schools. This fact shows the possibility of doing such work with undergraduates and with overworked instructors. It remains to show that the contrary practice of many schools arises chiefly from the circumstances already alluded to, or at least that the neglect of the new psychology in undergraduate work is not due to any constitutional defect in the methods of this science. The apparent difficulties are not insuperable. The most formidable one is doubtless the need of a laboratory. But a laboratory is also necessary for advanced work. A valid objection, however, is discovered in the time required for this work. Better supervision is required than for laboratory work in either chemistry or physics. This demands personal attention from the instructor in psychology. I think this objection is unanswerable. If instructors in psychology are unwilling to do this kind of work, we must wait until another species of instructor can be evolved.

It ought to be unnecessary to describe the effect on the student, of a laboratory course in psychology, and yet, like chemistry and physics and botany and zoölogy, this new science will have to fight for every inch of ground. The battle will probably take much the same general course. The first campaign has been won, and psychology may now have a laboratory for advanced workers. It is not my purpose to show that it is worth while to begin the study of all science by direct appeal to the original sources, nor shall I attempt to answer that valuable proposition concerning the necessity of book learning. We certainly cannot obtain all knowledge from our personal experience. It is generally recognized, however, as good policy to have some original ideas or personal experience before we depend much on books. There can be no defence of the introduction of the new psychology into beginning classes without practical work. Logic and metaphysics and the dictionary may be well taught without a laboratory; physiological and experimental psychology require some *things* to see and *feel*. Unless the spirit of the new method is breathed into the work, logic, metaphysics or ancient psychology may as well retain their place as introductions to philosophy. This spirit cannot be communicated to a large proportion of students by the most brilliant lecturer with a text-book as assistant. If it can be imparted in any manner, by any means, at any cost, it is worth serious consideration. It is said that only one student in ten receives much benefit from the required course in philosophy, and that students seldom elect additional work in this

line outside of lecture 'snaps.' Philosophy at least should be taught philosophically. No one denies that the most elementary principles of method are ignored by very many college teachers. Philosophy, perhaps more than any other discipline, is a sufferer from this cause. The self-activity of the individual is talked about in a more or less interesting manner, but there is often little successful effort made to start this activity a-going.

The writer does not believe, with some college professors, that philosophy is good only for the few. The introductory course in philosophy that does not force half the class into activity, and stimulate them to further systematic study, fails in its mission. Interest is of course essential; but interest that does not lead to inquiry, the interest that disappears with the professor's manuscript, is mere emotional debauchery.

Let us try to analyze the causes of the success of the new psychology as an introduction to the study of philosophy. Retaining our concrete form of exposition, we have first to note the kind of mental food the beginner in philosophy has been accustomed to. He is perhaps a junior. In every instance at least half of his previous study has been in language. Nearly half the remainder in mathematics. The other fourth has been given to science or to language, or in a few schools it may have been given to history. The first years in science do not always have the philosophical interest that might be attached to them if instructors were not bent on making specialists. Too often, though needless, the mechanical accumulation of facts, the unaided rediscovery of simple truths, 'discipline' blinds for a time the learner. In nearly all cases he is not led to connect his new knowledge with himself. This is not a fault of science, but is the misfortune of some teachers of science.

Language teaching is recognized as especially prone to dwell on the form and to neglect the substance of literature. It sometimes becomes painfully inhuman if not fiendish in its neglect of heart and soul. History is certainly alive. It is, moreover, at least anthropoid. The first years, however, are usually devoted to times having least in common with our own. It is difficult for the young student to put himself in place of the common citizen of Peiræus or Ostia. The generalizations of history are especially difficult because of the complexity of conditions and the social and political inexperience of the learner.

The junior, therefore, comes to psychology with considerable mental discipline, with some formal knowledge of the world's history, and with more or less information concerning isolated facts of several sciences.

A general course of physiological and experimental psychology with laboratory practice ought not only to add to his store of isolated facts, or even merely to impart additional mental culture; the opportunity for philosophical instruction must not be lost. The needed facts of the associated sciences will be brought together; their relations will become clear, and gradually there will grow up a rational appreciation of the interdependence of the forces of nature. Everything now points to man and to me. Its humanity is, of course, the chief characteristic of philosophy. The new psychology would fit the beginner to understand that all the universe is akin. This is the first opportunity the general student will have for bringing together many results of his previous study. Psychology is better fitted than any other branch of philosophy to suggest and to direct this reorganization, because it furnishes very many of the necessary facts, and does not plunge the inquirer too suddenly into bottomless speculation. I do not think it desirable or possible to *exclude* metaphysics from this introductory course, but the material is very largely positive, and the transition may be made so gradually that students will naturally *grow* the wings of generalization.

Besides furnishing essential materials for the further study of philosophy, this introductory course should familiarize the student with the characteristics of philosophic thinking, and, above all, should test his taste for work of a similar nature. If treated as an *introduction to philosophy*, and not as a course in philologic encyclopedism, or as special preparation for specializing in a specific specialty, the first year in the new psychology ought to stimulate *many* students to further work in related lines.

It therefore should be recommended to juniors and should occupy about one-fourth of their time for a year. At least two hours per week should be spent in the laboratory. The class work should not be formal and impersonal lectures, nor should it be time-killing oral quizes. Informal conferences, mutual quizes and explanations should be accompanied by frequent *written* reviews. If possible the *instructor* should be suppressed and the director and inspirer brought into his place. Such a course affords sufficient opportunity to become acquainted with the work of at least half a dozen modern writers besides a score of classic monographs. Yet, with the laboratory work, this is more than the introductory course in philosophy usually accomplishes.

H. K. WOLFE.

UNIVERSITY OF NEBRASKA.

A REJOINDER.

I regret very much that my review of Dr. Hodgson's address should seem to him unfair or lacking in courtesy. In his reply he is good enough to say that what he regards as a misstatement of his reasoning on my part was of course unintentional. Had I to re-write the review I should with equal courtesy state that Dr. Hodgson's securing his conclusions by 'blowing both hot and cold in his premises' was unintentional, but at the time I wrote this fact seemed to me so obvious as to be undeserving of mention. Dr. Hodgson set out with a determination to make 'truth the paramount consideration.' He has, however, as I cannot but believe, reasoned badly; and after having carefully read his reply and re-read his address, I fail to see that I have done him injustice. If he dislikes the expression of which I made use in describing his reasoning, I very willingly withdraw it, for it was no part of my purpose to cause needless pain to a writer whose works I read with both pleasure and profit. To me the phrase seems a harmless one, and much less energetic than the expressions of which he has himself made use in commenting upon my review.

As to the three confusions attributed to me: the careful reader of page 99 of the January number will notice that I nowhere confuse Mr. Hodgson's 'material world' with his 'universe.' That part of the universe beyond the limits of the known world of matter I call the 'world of faith.' Mr. Hodgson has himself called it an 'unseen world' (pp. 16, 17, 18), and has stated that belief in it is 'a state of mind which we call Faith' (p. 18). In the part of my review to which Mr. Hodgson objects it is treated precisely as it is in the part which he finds appreciative and generous, and for which he thanks me. Of the nature of the 'material element' which fills time and space in this world of faith I have said nothing.

The careful reader will notice, in the second place, that I nowhere confound Mr. Hodgson's 'material element in consciousness' with his 'matter' or 'material world.' Indeed, when I use the word 'matter' to indicate the material element in consciousness, I put it in quotation marks, and when I use it in the ordinary sense, as in the phrase 'the world of matter,' I do not do this. At one period of my life (with some shame do I confess it) I was a thorough Kantian, and I spent too many years in the company of 'matter' and 'form' to be capable of so gross a blunder. I am quite sure that an attentive reading of what I have written will reveal that I have not made this blunder.

As regards the third confusion—that between Mr. Hodgson's 'perceptually known' and his 'positively known'—here we have the very 'Kern' of the 'Pudel;' and in his use of these terms lies, as I believe, the very head and front of Mr. Hodgson's offending. I saw, of course, in reading his address, that he did not make the two strictly synonymous, and I have nowhere indicated that he did. I saw, however, with equal clearness, that to make them synonymous when treating of the 'formal' element in consciousness, and to distinguish between them when treating of the 'material' was an inconsistency which one might not unjustly characterize as 'blowing both hot and cold.' That Mr. Hodgson has been guilty of this inconsistency I can best show by giving a brief sketch of his argument.

He begins (Section II) by analyzing consciousness into its material and formal elements, concluding, as a result of this analysis, that infinity must attach, in the last resort, to time and space alone (p. 6). Section III is taken up with a digression on the nature of intensive quantity. In Section IV he returns to what he declares to be the main question of the evening, the conception of infinity, and sets before himself the double task: (1) of ascertaining its meaning, 'and consequently the nature of time and space in respect to infinity;' and (2) of discovering 'to what other things the attribute of infinity may be found to attach, in virtue of their connection with time and space, and what views of the Sum of Things are imposed upon us in consequence of the relations so disclosed' (p. 8). He concludes here that 'space and time in their entirety are wholly limitless and inexhaustible' (p. 11), since it is impossible to perceive or imagine a limit to or in space or time without perceiving or imagining it as having space or time on both sides of it.

Section V points out at length that the infinity of time and space are 'facts of perception,' not conceptions. 'We observe them,' he declares, 'alike in the minima and maxima of perception' (p. 11); 'what thought does with them is to make them objects of conception, out of, and as well as, objects of perception' (p. 12). A conception is declared to be a man-made entity in contrast to a perception independent of conceptual thought (p. 13); the conception of infinity is our gathering up of the facts constituting our perception of infinity into a single conspectus (p. 14). Thus Mr. Hodgson makes the infinity of time and space facts of perception—perceived facts. We do not infer or assume that time and space are infinite, but we perceive it, if, indeed, the above statements and others like them mean anything at all. Surely I am justified in thinking that Mr. Hodgson admits perceived facts to be worthy of

the name of positive knowledge, and that time and space 'perceptually' known as infinite are 'positively' known as infinite. The passage I shall quote just below will make clear that he does not regard the infinity of time and space as an article of faith.

This brings me to the concluding section of the address, which discusses the view of the universe involved in taking 'the perceptual facts constituting infinity as a disclosure, as far as they go, of its true nature.' It takes up, in other words, the second part of the task which, in Section IV, Mr. Hodgson set before himself. He reasons (p. 15) as follows: "Next, then, let us * * * * * see what follows from that view of infinity which I have endeavored to lay before you this evening, infinity as depending on perceptual facts. Since on this view the Universe is known to us only as existing in time and space, and these are known to us as extending beyond any boundary which we can conceive or imagine; and since, moreover, time and space are known to us, each in its kind and province, as inseparable co-elements of existing things, it follows that we must conceive existence as extending commensurably with time and space, beyond the boundaries of existence as positively known or imaginable by us. For in the infinity of time and space is involved their existence beyond the limits of any content positively known to us, and in their existence is involved that of some co-element or other, though not positively known to us, seeing that it is only as a co-element that we know them. You see how important are the two facts, disclosed only by analysis, first the illimitability of the formal element in consciousness, secondly its inseparability from its material co-element."

From the above I think Mr. Hodgson's mode of procedure in getting his world of faith is clear enough, as is also the defect in his reasoning. He finds in consciousness two inseparable elements: one of them is illimitable (p. 16), perceived to be illimitable (p. 11 ff.), *known* to be illimitable (p. 15). Since the two are inseparable, he argues, the other must be illimitable too, though we do not positively know it. In my review I pointed out that our positive knowledge ought to extend just as far in the case of the one element as in that of the other. Are they not inseparable? And if the infinity of space be a perceived fact, must not the infinity of that which fills the space perceived (I do not say *matter*) be a perceived fact too? Space and time, says Mr. Hodgson, cannot be perceived alone. Surely consistency would require Mr. Hodgson to maintain that the universe is *perceived* to be infinite, and that, if there be a world of faith, it can at least not be assumed as an indispensable

co-element to fill empty space and time. According to Section II of the address there can be no gap to fill—'this occupation of what I will call *the formal element* by feeling is essential to consciousness.' Were there, by any possibility, an empty space or time to fill, Mr. Hodgson's argument would lapse from the mere fact that the material element would thereby be proved not to be an indispensable co-element, and inseparable from the formal.

In his reply Mr. Hodgson admits that, when we perceive space and time to be infinite, 'some kind of material element in consciousness' shares their infinity. This admission is fatal to the argument. We have here a co-element already given, and none need be assumed. Is space perceived to be infinite? then so is this. Mr. Hodgson evidently thinks that such a co-element will not serve the purpose, and faith must provide another. His reason for this I think I can guess. His 'perceptual knowledge' does not really mean perception, although he has (Section V) treated it as such. When I perceive a chair, the content is as unmistakably perceived as the form. My knowledge of either element is equally positive. Can we in the same sense say that we perceive space and time to be infinite? Are we talking of genuine perception? Do we really *perceive* either the space and time or the material co-element that shares their infinity? Surely not. 'Perceptual knowledge' of the infinity of space and time I believe to be a delusion and a snare, and I believe it would conduce to clearness to avoid the expression altogether. It does not concern the question of the consistency of Mr. Hodgson's argument to enter into a discussion of the infinity and infinite divisibility of space and time. For this reason I omitted the discussion in my review of the address, and for the same reason I omit it here, merely referring anyone who may feel a curiosity upon this point to what I have printed elsewhere.[1] That perceptual knowledge does not really mean perception, even to Mr. Hodgson, appears to me evident from the language of his reply. He defines a 'perceptual knowledge' of time and space as a *representation* of them as filled with some material element in consciousness; and in what precedes he uses, as though they might, in his argument, be made to stand for each other, the expressions 'perceived,' 'represented in thought' and 'thought of as existing.' The inconsistency of Mr. Hodgson's reasoning lies, as I have already pointed out, in the fact that his 'perceptional knowledge' is made equivalent to perception and gives positive information in the case of the formal element in consciousness, but

[1] 'The Conception of the Infinite,' Phila., 1887. 'On Sameness and Identity,' Phila., 1890, § 36. 'The Philosophy of Spinoza,' 2d Edition. N. Y., 1894, p. 274, ff.

has a different meaning and fails to give such information in the case of the material element. The inconsistency stares one in the face.

In the last section of his address Mr. Hodgson has inserted two arguments for his world of faith, which form no part of his main discussion, and might very well have been advanced by one who had no sympathy with his ideas on infinity. The one is, that the world of matter does not furnish answers to all the questions it raises, and that these answers may be found in a world beyond it. The other is that there may well be modes of existence of which our present senses can give us no information. Upon these arguments it is unnecessary for me to dwell, for they have, as I have said, no necessary connection with the 'infinity and co-element' argument which I have criticized. G. S. F.

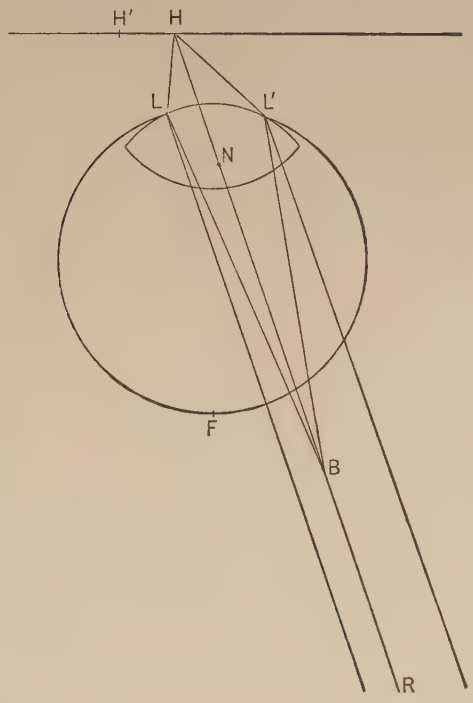
PHILADELPHIA.

SHADOWS OF BLOOD-VESSELS UPON THE RETINA.

If light is admitted into the eye through two small holes in diaphragm held in the front focal plane, two images of a blood-vessel in the front part of the retina are cast upon the sensitive layer. If now red and blue light are admitted together through the hole, the two shadows cast by the red light are farther apart than those cast by the blue light; and Prof. König has accounted for this fact by supposing that the layer of the retina which is sensitive to red rays is farther behind the blood vessel than that which is sensitive to blue rays, and, in fact, is behind the rods and comes altogether and in the pigment epithelium. Schapringer[1] says that the fact can be accounted for more simply. His argument is this: The point at which rays proceeding from a source of light are brought together is in the line joining the source of light with the nodal point of the eye (HN in the figure), and all the rays which reach this point are within a cone whose apex is this point and whose base is the pupil of the eye. For a source of light near the eye, this point is behind the retina, and farther behind for red rays than for blue. Now, the apparent position of the source of light is determined by the centre of its diffusion-circle, and the centre of the diffusion-circle will be farther away from the axis of the eye than is the point in which the line HN meets the retina, and the more so

[1] *Findet die 'Perception der verschiedenen Farben in ein und derselbe Lage der Netzhaut statt?* Pfl. Archiv., LX, 296–302.

the less refrangible are the rays of light. The virtual position of the image of H will, therefore, be farther from the axis if H is a red object than if it is a blue object; and the more so, again, the farther the bright point H is from the axis of the eye—the line FN produced. What follows from this, with perfect rigor, is that two *objects*, H and H′, will seem to be farther apart in red light than in blue. Dr. Schapringer takes it for granted that this is the same thing as to say that the *shadows* cast by a minute object in front of the nervous layer of the retina will be farther apart in red light than



in blue. He says: "Hence in red light the shadow falls on a spot farther from the axis than in blue, and this is the reason why with two holes the shadows are farther apart in red light than in blue." This is, in the first place, very inaccurately expressed; the red shadows might both be farther from the axis than the blue shadows, and yet not be farther apart from each other; what is necessary is that the shadow from the point H′ be *more* farther away than from the point H. But overlooking this point (which is a mere slip of the pen), Dr. Schapringer's argument seems to involve a substitution of dissimilars. The position of the shadow has nothing to do

with the centre of the diffusion-circle, but is determined solely by the direction of that ray of light which has been intercepted by the blood-vessel. When the point H is in the front focal plane of the eye for red, the red beam of light is a cylinder, and the blue beam of light is slightly convergent. (The convergence is exaggerated in the diagram.) This convergence, Schapringer says, would be so slight that it may be neglected, and he accordingly makes the blue beam of light also a cylinder, but *proceeding in a different direction* from the red beam. It would seem that in doing this he throws away as insignificant the very difference in refrangibility in which the cause of the phenomenon must be sought, and at the same time assumes a difference in direction of the two beams which does not exist if they are both taken to be cylinders. Even if the beam of red light be taken to be a cone, then the case would be different according as the distance apart of the two holes, H and H′, is such that the shadow-casting blood-vessel is in the right-hand half of each beam, the left-hand half of each, or in the right-hand half of one and the left-hand half of the other ; the red shadows would be farther apart than the blue or nearer together, according as one or the other of these conditions prevailed, or they might even happen to be equally distant. And it would still remain to be shown that this source of difference, if in the right direction, was sufficient in amount. Prof. König has as yet published only a brief sketch of his work, and the exact details of it are consequently not yet known.[1] It is, of course, impossible to suppose that he has not himself taken account of the difference of refrangibility of red and blue light. At the same time the reasons against his conclusion are so strong (this REVIEW, II., p. 144) that any simple means of accounting for the fact which would hold water would be very welcome.

C. L. FRANKLIN.

A COMMUNICATION.

Certain points emphasized in Prof. Armstrong's kindly notice of my book, *Philosophy of Mind*, enable me to make two or three desirable explanations.

In the first place, it is regarded as a defect, which increases he difficulty of understanding fully some of my positions, that the theory of knowledge postulated "is used as a kind of universal,

[1] When measurements are made the red and blue lights are used separately, but the accommodation of the eye would seem to be kept unchanged from the fact that the other eye is engaged in reading distances upon a scale.

with little or no systematic determination of subordinate criteria." This statement is partially true. But the answer to it, as a charge against my method or my conclusions, might be given in these very commonplace words: No one can say everything at once, and everyone has a certain right to follow his own notions of the best or the most convenient order in the discussion of connected topics on philosophy. In treating of the metaphysics of mind, I have simply postulated a general noëtical principle—which Prof. Armstrong himself admits is '*in some sort*' true for all those who believe in the possibility of knowledge as implicating reality—and have then at once proceeded to inquire, *what* do we know of that particular reality we call 'the mind,' and of its actual relations to the body and to external nature? Of course, those who take the position of the consistent positivist (if any such there be), or the deniers of all ontological science whatever, cannot accept my argument. For them there is no such possibility as a philosophy of mind, properly so called. But the 'systematic determination' of this vague general premise in noëtics is itself the complete theory of knowledge. Therefore, if at all by me, this defect of the *Philosophy of Mind* will have to be supplied later and in a separate treatise. I hope then to make it clear that the 'intermingling of the psychology and the noëtics of self-consciousness,' in discussing the metaphysics of mind, is no defect of any author's treatment, but an unavoidable result of the very nature of the subject treated.

Again, Prof. Armstrong finds my metaphysics 'familiar almost at a glance.' He declares it to be 'distinctly Lotzean in tone." Now, the measure of truth in this declaration depends upon how far we agree in understanding Lotze and upon how Prof. Armstrong understands me. As for myself, after considerable study, I confess I cannot understand Lotze on many most important points; and on many others, where I think I do understand him, I cannot agree. But metaphysics, in the general sense of the term, is another subject on which I have as yet publicly espoused and incorporated into this particular metaphysical treatise, only certain general principles, to which every one who has any metaphysics whatever will agree, I think, when he does something more than merely 'glance at' my words. If the latest critic of Lotze interprets him correctly in the following sentence, then I certainly am not distinctly Lotzean.

According to Prof. Jones' conception of Lotze's views, "Ideality and reality are handed over respectively to the thought and to its object; so that thought is ideal *only*, and objects are real *only*, or thought is ideal without being real, and its objects are real without

being ideal." Or, again: "Just as the real world takes no part in the thinking process, so the results of that process, the conceptions, classifications, judgments and influences, are not copies of reality, nor do they in any way represent really existing facts or events." It would be difficult to gather into two sentences of the same length views more unlike my own, as to the nature of both thought and reality, than these Lotzean utterances—if, indeed, they are to be so characterized.

In general, may I not utter a mild—and, I presume, it will turn out ineffective—but earnest protest against being '*classed*' in any such fashion? Without pretence of independence or of originality, and with cheerful readiness to acknowledge and avail myself of the results of all successful research and sound thinking on the part of others, I much prefer not to be named after any one. The very motto I have chosen for this book is my protest. In this connection I will improve the opportunity to say that those critics who have spoken of my *Elements of Physiological Psychology* as modeled after Wundt, etc., simply go contrary to the facts. When I began my work in physiological and experimental psychology, now about sixteen years ago, I had never read Wundt's work (then only some four years old); I am not sure now that I even knew of its existence. The *Grundzüge*, and all the other work of the Leipzig laboratory, have always been used by me as any other work—making allowance for differences in quality, quantity and range. But I suppose that some of the younger men, who have been trained in the laboratories which have sprung up since I began to investigate, have difficulty in conceiving how any one could get at any truth in other ways than those to which they have been accustomed, or could venture an opinion within the mystical and exclusive circle of their own specialties.

I will speak of only one thing more in explanation. Prof. Armstrong complains, not bitterly but mildly, of my polemical style in certain chapters of my last book; and he rebukes me for making use of 'horrible examples.' On this point I will only say that, in order to discuss three or four views which I considered mistaken, I selected the most distinguished and respected examples of these views; and if in treating them polemically I was in any degree unjust, or in any place discourteous, I desire once for all humbly to apologize. Contrary to my usual custom, I adopted deliberately the polemical method as best adapted for discussing certain topics in this 'technical treatise.' My judgment as to the wisdom of this course I do not now withdraw. In general, I suppose there are

very few writers on psychology and philosophy who have used the polemical method so sparingly as I have ; and, on the contrary, taking my theological writings also into the account, there are very few writers who have suffered so much from it, or so unjustly, at the hands of others, and in every way.

In general, too, I sincerely hope that the subject to which I am devoted may soon pass from the stage of pseudo-critical characterization to the stage of really critical and thorough discussion. Just now I should particularly like to have my destructive discussion of the science and metaphysics of the principle of psycho-physical parallelism discussed in its turn. I really think this principle needs sorely at present the assistance and tender nursing care of its champions. If a challenge is urgently needed to call forth these champions, I am sure the challenge should be issued.

GEORGE TRUMBULL LADD.

A NOTICE.

Rev. W. W. Campbell, the joint author of Study A., against whom Prof. Münsterberg in his 'Notice' fears I have personal emnity, permits me to state that "he has read my discussion of the 'Motor Power of Ideas' and finds it a dignified and sober criticism, without show of enmity, and such as any student ought to accept of his work."

HERBERT NICHOLS.

PSYCHOLOGICAL LITERATURE.

GENERAL.

Comte, Mill and Spencer: an Outline of Philosophy. J. Watson. Glasgow, Maclehose; New York, Macmillan. 1895. Pp. vii+302.

The title of this book is a little misleading. Comte, Mill and Spencer are treated neither exhaustively nor comparatively nor on their own account nor exclusively; two names have at least an equal claim to figure in the title: Darwin, to whom nearly fifty pages are given—almost as much as respectively to Spencer and to Mill, and more than twice the space allotted to Comte—and Kant, the discussion of whose theories of morality, religion and art takes up fully a third of the volume. The aim of the book, as indicated by the sub-title, is to develop, in connection with the criticism of these five thinkers, a system of what the author designates as 'Intellectual Idealism.' For the main outlines of his system he rightly acknowledges his large and manifest indebtedness to Green and Edward Caird. Indeed, his book may be characterised as a systematic exposition in brief compass and in clear and intelligible language of the dominant British Neo-Kantian or Neo-Hegelian type of thinking. The method and the matter of exposition are both conformable to the traditions of the school.

The gist of the argument for the doctrine advocated is found in the chapter criticising Spencer's doctrine of consciousness. Spencer maintains that the very nature of consciousness forbids any transcendence of the distinction of subject and object; we are referred for the explanation of either to its opposite, but neither can be resolved into the other. The reply acknowledges the distinction, but seeks to show that the object is ultimately identical with the subject. The demonstration is as follows: No 'thing' is an isolated individual. All its determinations are expressions of relations to something else. The particular 'thing,' therefore, is a mode of existence as a whole. The object as a whole is, therefore, a systemated unity. Similarly of the organism: its bodily structure is related to the whole just like anything else. But its functions are inseparable from its structure. The total object is, therefore, not merely a systematic but an organic

unity. But the same considerations apply to feeling, in which the life of the organism and, therefore, of the whole world pulsates, and to the consciousness which feeling, furnishing its entire content, makes possible. The object is, therefore, not only a systematic and organic unity, but self-conscious intelligence or God (173).

The argument seems inconclusive. Granted that no individual—no bit of matter, no organism, no sentient or conscious being—is isolated, but that each is an aspect and mirror of the whole; granted, further, that man most perfectly mirrors the whole, inwardising in feeling, in a way, the whole of nature and conscious of this whole, in a degree, as he interprets the content of his feeling: does it, *therefore*, follow that the object is identical with the subject, and that the whole is one self-conscious intelligence? Hardly. It follows that the universe is of a sort to be mirrored in consciousness, and that it somehow includes the principle of consciousness in its nature; but it is a great step to the conclusion that the universe is itself self-conscious. This raises the whole question of the self. But neither at this point nor elsewhere in the book are the difficulties connected with this question rigorously dealt with. Professor Watson argues, apparently, that because each living being, each sentient being, each conscious being is in determinate connection with the whole, there is therefore a universal life, a universal sentiency, a universal consciousness. And because the existence of each preceding mode is a condition for the existence of the following, he concludes, apparently, that the lower is the possibility of the higher in the sense that it is essentially of the same nature. These conclusions may all be true, but they need more explicit mediation. The short and easy proof of Idealism is not enough. Nevertheless, as an outline of philosophy, it is doubtful if there is any other book in English that could be so cordially commended to young students.

SMITH COLLEGE. H. N. GARDINER.

An Introduction to Comparative Psychology. C. L. MORGAN. London, Walter Scott; imported by Chas. Scribner's Sons, New York, 1894. Pp. xiv+382.

Mr. Morgan's 'Introduction to Comparative Psychology' is an attempt to clear the ground for a science that has suffered thus far from the homocentric character of psychical analysis as comparative anatomy and physiology have suffered earlier from the almost exclusive interest that centered in the human organism.

With this end in view Mr. Morgan starts with the postulate of a monism. He is not, however, convinced of its necessity as a basis

for comparative psychology, and warns, in a note, all those to whom philosophic speculation has no attractions to pass over his prolegomena.

It is true that his subsequent analysis may be followed out from a dualistic standpoint. It is, for his practical analysis, a matter of indifference to Mr. Morgan whether physical and psychical be regarded as two aspects of the same entity or whether we conceive them as simply running parallel with each other. His thought is dominated by the analogy of the two aspects of the curve. It is certainly a matter of indifference to one who would study these two aspects, as respects their directions and mutual relations, whether he sees them as two aspects of one line or as the corresponding aspects of two lines that run parallel with each other.

It is in terms of just such tangible analogies that Mr. Morgan does most of his thinking. The whole treatise is built on three of them : (a) that already named between the two aspects of a curve and the parallel physical and psychical phenomena, (b) the analogy between a wave in an undulating medium and consciousness, and (c) that between our recognition of consciousness in another being and the inference a clock might make by means of the relation between its hands and its own inner works to the inner works of another clock from the position of its hands. These analogies cannot lay claim to any great novelty ; their value lies in their simplicity and lucidity.

Such analogies, however, are at best makeshifts. They inevitably carry with them much error because they are analogies, *i. e.*, concepts which, for the sake of concreteness, are but incompletely analyzed and abstracted. We are very definitely of the opinion that psychology, especially comparative psychology, needs, on the contrary, a thoroughgoing analysis of its fundamental concepts to put it upon its feet.

Experimental psychology fares, to be sure, about as well with the incomplete analysis as it would with the most searching, because the most that such an analysis could do would be to justify it in its use of the methods of physics and physiology. The theoretical stage in no science has done much more than justify and free the methods which had been worked out in its first period of discovery. Experimental psychology would profit greatly by such a freeing of its tools, *i. e.*, in the definite formulation of a psychological method as ultimately distinct from those of the physical and biological sciences. Still it suffers as yet no serious set-back through this lack of definiteness in its own territory. A capital error, such as is involved in 'Fechner's law,' will hardly be committed again.

But the case stands quite differently with comparative psychology. The experimental psychologist has the test of immediate experience for the distinction which he makes between the physical and the psychical. The reality of the distinction is justified by the success of the life processes that assume it. The experimenter has therefore only to follow rigidly the essential reactions that make up his life and he need not go astray. This is but another statement for the assertion that the distinction between the physical and the psychical is an immediate datum of experience. And one must go a step further than this; the distinction between the physical and the psychical in others is as really an immediate datum of experience. We are as essentially social beings as physical and physiological beings, despite the analogy of the clocks. (We must deprecate the reappearance of this spook of Paley's watch, after its having been laid in the field of natural theology, to haunt the domains of a modern science.) The development of the distinction between the physical and psychical in others proceeds *pari passu* with that in the child's consciousness of himself—if for no other reason because he could never form the conception of himself as psychical without the conception of others. Or again man is essentially social.

The experimenter therefore runs no more risk of making an unreal distinction between the physical and psychical in others than he does in his analysis of his own consciousness. But just in proportion as our analysis leaves the stage of self-consciousness within which we live, and approaches those points of civilization where we are no longer perfectly at home, and especially when we leave the human intelligence quite behind and strive to reconstruct the consciousness of the lower animals, are we at the mercy of dangerous analogies which were before harmless. These analogies, in their proper place, serve as illustrations, that is act as stimuli to reconstruct what in all its details is fully within our power. The analogy of the clocks may serve fairly well to recall to one the process by which he revises his social judgment—detects, as it were, a social hallucination. It is as far from copying the state of consciousness in which a dog recognizes a hostile intruder, as the click of a calculating machine would be from describing his angry growl at the loss of half his dinner.

The same criticism holds in regard to the analogy of the wave of consciousness. This serves excellently to recall the onward sweep of concentrated attention through the mass of details that crowd the field of consciousness, and the positions of relative

importance which those details hold. But while this illustration summons up the reality in my consciousness, it is but the most superficial analysis of attention. This has the unity and direction of the purposive act, not that of a wave propelled by a *vis a tergo*. So that at the point where we are forced to abandon the concrete reality of our own full reconstructions, the analogy becomes as false as the reconstruction of Greek life by the romancer of the Middle Ages. The Socratic task of substituting analytical definitions for illustrations is that which faces the comparative psychologist. In the opinion of the reviewer this can only be successfully met when the logical process which is the reality of the distinction between the physical and psychical has been recognized.

UNIVERSITY OF CHICAGO. GEO. H. MEAD.

L'Année Psychologique. Ie. année (1894). H. BEAUNIS and A. BINET, 1895. Paris, Alcan, 1895. Pp. VII + 619.

The value of this excellent publication will be evident at a glance. Besides the contributions made during the past year from the Laboratory of the Sorbonne—reported in full, and deserving later notice—it contains the fullest and most adequate report of the facilities, equipment of the universities, the details of courses of instruction given in the United States that has ever been published. This excellent paper is from the pen of Prof. E. B. Delabarre. The curious error of the translator—M. Binet himself—in rendering the letters of the degree 'Ph. D.' by Docteur en Médicine, is corrected by a note on p. 619. The final part of the book, devoted to a bibliography of the literature of psychology, neurology, etc., for the year 1894, seems to be full and accurate. It comprises 1217 titles: but quite a large number of these are from the literature of '93, and it is difficult to see sufficient reason for their presence. Compared with the recent *Index* of Warren and Farrand, it runs short about 100 titles by number, but seems to include more supplementary headings put in as sub-titles 'a,' 'b,' etc., throughout. It has more French titles, as one would expect, and fewer English, than the American publication. One very grave defect, however, of the French bibliography, in the opinion of the present reviewer, is that all the titles are translated into French. The main function of such a bibliography, one would think, is that of giving to those who consult it exact details of the title, so that it can be ordered from the booksellers or referred to under its title in libraries. But how can one write for a book and be sure his order will be understood if what he orders is only the equivalent in another language of the real

title? I myself have actually had this difficulty, or rather uncertainty, in ordering a title from this *Année*. Suppose a Frenchman ordering Ladd's 'Manuel de Psychologie,' (an imaginary case) which of the author's three books would he be likely to get? French readers, on the other hand, are not much benefitted by this preference for French: for if a man cannot read an English or German title he is not likely to look up the book all of which is in one of those languages. The remaining portion of the *Année* is an extended collection of notices of important pieces of work issued during the year, and seems to have been done with great care and labor. Of course opinions will differ whether the most important things under each head have been selected; but that is unavoidable where selections are made at all. On the whole, the compilers of the *Année* are to be congratulated and thanked. It is pleasant, also, to note their announcement that the publication is to continue annually.

J. M. B.

Lehrbuch der Psychologie. W. VOLKMANN. 4te. Auf. Ed. by C. S. Cornelius. 2 vols. Cöthen, Schulze, 1894–5. Pp. VII + 511, V + 568.

All psychologists will welcome the fourth edition of the great work of Volkmann. It has long been a standard work, and although always partial to the Herbartian wing in its citations of literature no less than in the theoretical positions of the author, yet it has had great value as a work of reference. It must be said, however, that the chief value of the work in the edition before us is to be found in the 'real essence'—the original work of Volkmann—rather than in what is added to it by the 'accidents' of editing. The prospectus leads one to believe that the citations of literature, which have always been an important feature of the work, are well up to date and very full. Anyone looking for anything of the kind will be very much disappointed. The additions made to the literature at the end of the chapters are marked with an asterisk, and they turn out to be relatively meagre in the extreme. Having an eye on the works in English added to the bibliographies, the inadequacy becomes extraordinary. Looking under Emotion, I find no reference to James, nor to the literature which his chapter has called forth. Similarly, there is no reference to that author in connection with the *Innervationsgefühl*. Indeed, the only reference I have come across—I have not looked over every page, however, with this in view—to James' 'Principles' is the citation of the title of the book in the register of literature at the end. Similarly a search for the

literature of the writers in England shows the defect about equally marked, Ward, Sidgwick, and others being largely overlooked. This seems to me to be simply the old case of 'nothing worth knowing outside of my own country,' and it is astonishing that the large sale of earlier editions of the work of Volkmann—if no other consideration availed—did not lead Prof. Cornelius to have the foreign purchaser somewhat in mind. J. M. B.

Introduction to the Theory of Science and Metaphysics. A. RIEHL. Translated by A. FAIRBANKS. London, Kegan Paul, Trench, Trübner & Co., 1894. Pp. VIII + 346.

It would be hard to find a book in the more recent Kant literature of Germany whose translation could be more generally welcome to English and American students than Prof. Riehl's 'Science and Metaphysics.' The German original of the work forms the conclusion of a larger work, 'Der philosophische Kriticismus und seine Bedeutung für die positive Wissenschaft,'[1] which seems not to be generally known in this country or England. The work, however, has been influential in Germany, and this translation of its concluding part into English is sure to increase its usefulness materially. Moreover, Prof. Riehl, unlike many German philosophers whose writings have been translated recently, can congratulate himself that his book has been rendered into readable English. It should be said, however, that Prof. Riehl by his own admirable style, by his unfailing clearness and precision, has done more to assist translation than some of his compatriots. The German original of the entire work was reviewed with greatest care some time ago[2] by Prof. Adamson, who paid high tribute to the author's 'completeness of knowledge and maturity of philosophical reflection,' and recognized the great value of his statement of Kantianism.

This translation comes at a time when interest in the relation of science and metaphysics is reviving, or rather when the relation is getting a positive character. Metaphysical philosophy and science were not always apart. "The separation," says Prof. Riehl (p. 12), "of philosophy and science which has resulted in the regard of them as antitheses dates back no further than the period which in Germany followed Kant. . . . . This antithesis of philosophy and science forms only an isolated episode in the history of thought, which to-day appears to be more than a passing phenomenon only because it is so near us in time, and which is to be explained from

[1] Leipzig, W. Engelmann, 1887.

[2] Mind, vol. 14, Jan., 1889, pp. 66 ff.

special causes, namely, the excess of æsthetic culture over scientific among the German people." The excess of æsthetic culture helps to explain the distinction between phenomena and things-in-themselves, a distinction which in its turn is the explanation of the exclusion of metaphysics from science. The things-in-themselves or noumena, removed by Kant so absolutely from the sphere of the proper activity of the (scientific) reason, became at once the object of purely philosophical or non-scientific inquiry. They were necessary to Kant's system, but it was unfortunate for his criticism, doomed to subsequent shocks in history, that he could not be quiet about them; to call attention to them was fatal. Kant, then, denied the right of enquiry to metaphysics; he reduced metaphysics to a body of presuppositions; the inquiring reason could turn only to natural science; but his criticism rather stimulated than controlled the reason. The first influence of his philosophy was to make both philosophy and science extreme, each after its kind, metaphysics rising in its flights to all appearances far above the supporting atmosphere of experience, and science becoming ever more forgetful of its metaphysical presuppositions. Recent tendencies of thought, however, show science actually becoming interested in its presuppositions and philosophy returning to something like its old character of 'natural philosophy.' But the change only justifies Prof. Riehl's declaration of the isolated character of the episode or the passing character of the phenomenon which Kant's philosophy reports, and one has to wonder that he who saw so clearly could remain so loyal to Kantianism. Yet was he altogether loyal?

In regard to Riehl's Kantianism, Prof. Adamson says in the review already referred to that the work of Riehl, although in the treatment of its subject "characterized by so much freshness and originality of conception, so comprehensive an insight into the relations of philosophical and scientific problems, and so close a reference to the general tenor of modern science, as to render it in no sense a mere re-presentation of the work already achieved by Kant," is still no real departure from Kant; 'the form of expression is different,' but the difference 'involves no matter of great philosophical significance,' Critical Realism, only another name for Kantianism, being quite as hostile to metaphysical inquiry. But I cannot feel that this is altogether fair to Riehl. We may have to style him a Kantian,—some people depend on epithets,—but there are tendencies in Riehl's book that are rather treacherous to the Critical Philosophy. Thus the Critical Realism rests upon a theory of the immediate perception of the external world, and this involves

a change not to be overlooked of the Kantian conception of scientific knowledge.

Riehl's idea of science is truer, I should say, to psychology, with its final dependence on introspection, than to ordinary physical or objective science; so that, if we put two and two together, he seems to say that the modern 'natural philosophy,' which has arisen or is arising with the passing of the antithesis between philosophy and science, has a psychological point of view, if not also a psychological content. This may be interpreting Riehl to himself as well as to others, but present tendencies in philosophy make the interpretation attractive. For through psychology, especially through experimental and comparative psychology, more than through any other lines of investigation, science has been brought to a consciousness of its presuppositions, or at least to a consciousness of the fact that it has presuppositions, and this consciousness is entering into scientific experience as an all-important factor. Prof. Ladd's recent book, 'Philosophy of Mind,' is at least in its ideal true to the tendency here indicated. It shows how science, in spite of Kant or Kantianism, was doomed to become metaphysically self-conscious as soon as it should be well within the sphere of psychology. A sentence or two from Morgan's 'Introduction to Comparative Psychology,' a book of perhaps more value for the air that it breathes than for the words that it speaks, may be quoted here: 'I do not think that the metaphysics of the subject can be avoided in any such inquiry [as that of 'mental evolution in all its aspects.'] It is not a question of metaphysics or no metaphysics, but of good metaphysics or bad.' Metaphysics was once the original sin of the human reason, whose salvation could come only through the grace of a purely non-metaphysical science, but in these days original sin is a doctrine, not a fact, and a doctrine as untenable in psychology as in religion.

In conclusion let me call attention to the most suggestive turn that Prof. Riehl gives to Kant's *a priori* with its criteria of universality and necessity. He gives to the *a priori* a social function in experience. He would substitute for the explanation of knowledge from the point of view of the individual its explanation through society. "I am inclined," he says (p. 66), "to believe that with every perception by man is associated the impulse to communicate it. Experience is a social concept, not a concept of individual psychology." The bearing of this upon the psychology of language is evident. It also brings psychology and sociology to what is almost if not quite an identity. It points the way to relating the psychology

of Kant at once individualistic and rationalistic to the psychology of to-day with its double interest in 'ontogenesis' and 'phylogenesis.' Its special meaning for Riehl appears in later chapters, notably in the very valuable chapter on 'Determinism of the Will,' Part II, ch. III.

A. H. Lloyd.

University of Michigan.

SOCIAL PSYCHOLOGY.

(1.) *La Sociologie: ses conditions d'existence, son importance scientifique et philosophique.* M. Bernès. Revue de Métaphysique, Mars, 1895.

(2.) *Sur la méthode de la sociologie.* M. Bernès. Revue Philos., Mars and Avril, 1895.

In (1) M. Bernès presents an eminently judicious and philosophical view of the object and method of sociology, and in (2) he applies his principles to a criticism of Durkheim's articles noticed in the Review for May. The two dominant characteristics of contemporary sociology are declared to be (a), imitation of natural science (especially biology), and (b) absolute opposition to subjective (psychological) sociology under all its forms, in particular the refusal to attribute any *rôle* to the reflective will of the members of society. These two characteristics are sources of fundamental errors. To make sociology purely 'objective' is to deprive it of its essential character; and just in proportion as we make it abstract, objective, mathematical, are we in danger of leaving out the distinguishing mark of intelligent action. To give any meaning to economic, legal or political institutions of the past, we have to think ourselves into the state of society which they express, to interpret them by internal psychological causes. The common fault of both subjective and objective sociology is to identify the subjective with the individual; whereas by looking within we may pass the bounds of individuality as truly as by looking without. The society of which we form a part is within us as truly as we are in it. Sociability, more or less conscious, is as truly an element in the social reality of the present as is any objective social phenomenon. On the other hand, there is no ideal but in relation to a reality already existing; the existing solidarity is the occasion for the subjective practical or moral appreciation of things or actions which in turn becomes an important factor in future development. For society is a 'becoming' as well as a thing. Every collective aspiration which by its realization results in consolidating the group, in making it at once more complex, more

plastic, more conscious of itself, becomes thereby a cause of progress. In proportion as society is better distinguished from every other reality and corresponds better to its definition,—in proportion, that is, as it creates itself—it becomes more completely an object of science; but of a science whose laws are at once objective and ideal, expressing relations between what is already in existence and that which, while not yet existing, has already begun to be.

The most important addition made by (2) to the conceptions above noted is that of the psychological and social significance of action as relating the individual and the social. On the one hand, every state of consciousness is already an action, at least by anticipation; action is a principle of expansion which sets us in some fashion outside ourselves, and would cease to be action if we could make it entirely individual and internal. Thus by action the psychological and subjective life takes on already a social value. But, on the other hand, action is not wholly comprehended within the subjective idea of an internal principle of activity; it exists only by virtue of a sum of objective conditions which give it a body and a form, and without which it would be reduced to a mere potentiality—that is, to an abstract and fictitious entity. Action is thus the most elementary datum of social psychology. It is the bond between subjective and objective, between actual and ideal. It is the concrete fact from which these opposing terms are derived.

University of Chicago. J. H. Tufts.

CRIMINOLOGY.

Naturgeschichte des Verbrechers. Grundzüge der criminellen Anthropologie und Criminal-psychologie. H. Kurella. Stuttgart, Enke, 1894. Pp. 284.

Dr. Kurella, the author of this 'Science of the Criminal,' is the medical director of the insane asylum at Brieg, in Silesia, where he has had good opportunities for the study of criminals. During ten years he has carefully examined many hundreds of cases of insane criminals. He has also carefully studied the somewhat abundant literature on criminology that has been published during the last few years. As a result of his observations and reading he has become deeply impressed by the theories of Professor Lombroso of Turin, and in his introduction declares his enthusiastic adherence to the theory advanced by Lombroso, that there is a certain peculiar and distinct type of irreclaimable criminal, described as *delinquente nato*, who is, so to speak, fatally predetermined to crime, being the genuine born criminal.

The author thinks that sufficient observations have been made and results published to form the basis of a work giving the fundamental principles of criminal anthropology and criminal psychology. He believes that such a work will be specially useful to doctors consulted about criminals, to psychiatrists, judges, and all deeply concerned with the problem of the treatment of criminals. He therefore proceeds to give a summary of the chief results compiled by many recent writers on criminology. He specially mentions Prof. Lombroso, Turin; Prof. E. V. Hofman, Vienna; Dr. Sommer, Allenberg; Prof. Benedict, Vienna; Dr. Mingazinni, Rome; Prof. Dr. v. Tschisch, Dorpat; Dr. J. Karlowicz, Warsaw; Dr. Wesnic, Belgrade. In his register of authors consulted, we find some 162 names, some of whom have been cited as frequently as thirty-five times. A work of 284 pages, giving an epitome of the tabulated results of so many writers, cannot be summarized in a brief review.

To show the need of a more extensive study of criminals, he gives a mass of statistics to prove that crime is greatly increasing. It occurs to one in reading these statistics to ask, Are these conclusive evidence of the increase of crime, or of increased vigilance in the detection and punishment of crime? Do we mean by 'criminal' those who are arrested for crime? And the suspicion arises that statistics are being used uncritically, and that the examination of the meaning of 'criminal' is neglected. We think this distrust will increase as the reader proceeds.

I. *Criminal Anthropology.*—The author first treats of anatomical peculiarities of criminals. Diagrams and explanations are given of methods of craniometry. Tables showing characteristics of criminals in the general shape of the skull and structure of particular parts. Exact measurements corroborate the common suspicion of the low and retreating forehead. A list is given of marks of degeneracy or atavism frequently found. A detailed reference is given to structural varieties of the following: the ear, breast, sexual organs, beard, hair, excess in number of fingers, etc.; arrested development of organs; acquired characters, *e. g.*, tattooing.

II. Under 'biological factors,' he discussed nourishment and digestion, susceptibility to feeling, motor characteristics. Under 'Heredity' he gives interesting tables of criminal families through several generations (or degenerations!), showing the interconnection of nervous diseases, pauperism, alcoholism, insanity, prostitution, suicide, etc. The condensation of the book may be judged from the fact that, after referring to Dugdale's well-known 'The Jukes' as 'the most valuable contribution to criminal heredity,'

its results are presented in little more than half a page. He next discusses the criminal 'milieu.' Then gives a 'physiognomy of criminals,' with illustrations that remind one of Lavater. In nearly every case we notice smallness of head, or at least smallness of the fore part of the head. Frequently we see the following: Large under jaw and high cheek-bones; prognathic, platycephalic, skaphocephalic, parietal and occipital regions of skull large with small frontal region. Deformities of ears and lips. One particular form of ear is regarded as peculiarly indicative of the criminal. It seems that Mozart had this 'fatal ear.' His aberration from crime to music is explained by saying that this is not the only example of a close affinity between genius and degeneration; adding that the same excessive development of the sense of hearing, indicated by the extra development of the external ear muscle, is common to the musical genius and the burglar who partakes in the plundering instincts of the carnivora with the necessary accompaniment of acute hearing.

III. *Criminal Psychology.*—The author first discusses the theory of 'moral insanity.' He prefers Lombroso's assumption of the born criminal—*delinquente nato*—having the following peculiarities:

(1.) Parasitic tendencies very pronounced. (2.) Honor and truthfulness, regarded by the author as late products of civilization, are utterly lacking in the criminal who reverts to an earlier type. (3.) An interesting account is given of the traditions, codes and vagabond-slang-language of criminals.

IV. Under moral concepts and passions, he says: There is no thought of the future in the criminal; he acts in accordance with the passion of the moment; consequently all attempts to prevent crime by threats or by making an example of convicted offenders are futile. The criminal utterly lacks sympathy and pity. He is reckless, cruel, lazy; despises work as beneath his dignity, prides himself on being a criminal, craves notoriety, and desires to become a 'virtuoso' in crime. The author outlines a psychology of Ethics of Criminology, and quite significantly accepts Mr. Herbert Spencer and Lombroso as his authorities. Everything must be explained by feelings. A sentence will indicate the author's standpoint: "Now, the most superficial observation of children—and no one has shown this more clearly than Lombroso—will make evident that pity and the feeling of right are in the first place acquired, and in the congenital lack of these feelings the criminal is like all children" (p. 250). It is a pity the author did not make even a superficial examinatisn of children instead of giving us the dicta of Lombroso.

Would not a very little observation and *reflection* make evident that the child, not being self-conscious, is neither positively selfish nor positively unselfish, but simply *non-self-ish.* Surely the instinctive actions of the child would be less incorrectly regarded as social than as aggressively anti-social—which is the meaning of lack of pity in the case of the criminal. We confess that we are deeply disappointed with the chapter on Criminal Psychology. There seems to be evidence of a very slight acquaintance with the general psychology, psychology of ethics and theory of ethics of the normal type ; not only so, but there are references of contempt for those branches of study. Yet the author has followed recent discussions sufficiently to cast in his lot thus: "The psycho-physiological theory of moral conduct, as also of crime, is dependent on the psycho-physiological theory of 'affect.' The ordinary psychologist devotes altogether too much attention to the examination of the intellectual side of consciousness ; the student of criminology cannot thus ignore the passive 'affect'" (p. 252).

In concluding, the author returns to theoretical discussions. He says : All scientific inquiries into man's conduct must necessarily assume the deterministic theory as the correct one. If there can be any scientific account of the criminal's conduct at all, Lombroso's theory should not be objected to on the score of its fatalism (p. 263).

The author describes his work as an outline of methods, results and fundamental principles in criminal anthropology and criminal psychology. He admits that there are two schools, the Italian school, following Lombroso, endeavoring to establish heredity as the full explanation of the genuine criminal, whom they term *delinquente nato ;* excluding from the explanation the influence of environment and the results of educative forces ; the other, the French school, giving a large place to the influence of environment and the significance of educative forces in their explanation of the adult criminal.

Now, in a general outline of methods and results, we naturally expect to see the results and arguments of both sides fairly and fully presented, and then a decision given in favor of the one regarded as most satisfactory. But Dr. Kurella announces at the outset that he has adopted Lombroso's position, stands forth as an advocate and defender of his assumptions, and then gives tabulated results, mainly selected from Lombroso and those writers who agree with him. This it seems to us, is not giving an outline of criminology, but rather the details of a defense of the theory of one party of criminologists. Almost anything may be proved in this way if one may be allowed to select the facts and neglect everything

that does not fit the theory espoused. It is significant, too, that the cases examined personally by Dr. Kurella were insane criminals. Surely we need a more scientific selection under the term 'criminal.' When we examine the accumulation of evidence and argument brought forward to establish Lombroso's 'born criminal'—*delinquente nato*—we find that it is all based on the examination of adult criminals who have become habituated to crime. Lombroso and Kurella, viewing these adult (and sometimes insane) criminals with settled tendencies and formed habits, conclude that they are impervious to social reformatory influences; then they conclude that they must have been at birth incapable of education to good habits, fatally predetermined from the first to evil and to evil only. Everything in connection with their character is to be explained from heredity, nothing allowed to environment and educative efforts.

This, it seems to us, is a fallacy in the speculative sort which Dr. Kurella is fond of deriding. How can they rule out the influence of environment and education when, as a matter of fact, these criminals have been in an environmemt and under a training towards vice? If these criminals, tabulated, had been in a contrary environment and under proper social influences from birth, and then in spite of this became irreclaimable criminals, there might be some plausibility in the view that the genuine criminal nature is inborn and is utterly incapable of being essentially modified. When it comes to Lombroso's theory, however, Dr. Kurella seems to be as fatally predetermined to it as the criminal *delinquente nato* is to crime. Bring forward any number of cases of reformation of hardened criminals, he calmly rejoins: we never denied that ordinary criminals might be reclaimed; these were not criminals *delinquente nato*.

In spite of these defects in theory, the work is one that should be widely read, not only by those to whom the author appeals, but also by psychologists and moralists. It will excite many new thoughts, suggest new phases of old problems, and help to indicate how wide a field still awaits the trained psychologist and moralist in the investigation of the psychology and psychology of ethics of the criminal.

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On the New Use of Some Older Sciences. CHARLES L. DANA. Reprinted from the Medical Record, Dec. 15, 1894. New York, Trow Directory Co. Pp. 19.

Dr. Dana's article is a summary of the results obtained by Lom-

broso, Féré, and others, from anthropometric examinations of criminals. In addition to the well-known facts about the shape of the skull and the facial expression, he discusses a number of minor marks: the shape of jaw and cheekbone, the ridge along the centre of the palate, the shape of the ear, length of third finger, prehensile foot, etc. The author differs from the Italian school in classing together criminal, insane and neuropathic degeneracy and refusing to consider these as different types. From an ethical standpoint the presence of these marks of degeneracy in an individual "throws an additional responsibility upon him. * * * We do not excuse the cripple who attempts to become a sprinter, nor should we excuse the morally defective who * * * fail to husband the endowments they possess." A few such sentiments as these will go a long way toward justifying this science in the eyes of those who are accustomed to regard it as an example of *fin-de-siècle* morbidness.

H. C. Warren.

NEUROLOGY AND BIOLOGY.

Il Cervello in relazione con i fenomeni psichici. G. Mingazzini. Turin, Fratelli Bocca, 1895. Pp. 204.

This interesting study on the morphology of the cerebral hemispheres of man forms the twenty-second volume in a 'Bibliotheca Antropologica-Giuridica,' a series to which the best known of the Italian workers in this field have already contributed. The problem attacked is the one which dates at least from the days of Erisistratus—namely, the interpretation of the gyri of the cerebral hemispheres.

To approach this question properly the comparative anatomy and embryology of the primate brain is needed; and assuming both interest and knowledge, the author attacks his subject without further preface, comparing the fœtal human, with the fœtal primate brain, so far as material will permit. The relation found in the length, angle, position and connections of the important sulci is such that, in those primates more removed from man, there is far more divergence from the human type than exists between man and the highest primates. In ontogeny the similarity in development becomes less with the increasing age of the individuals, and soon any form begins to exhibit those features which are distinctive of it at maturity. Thus the characters of the cerebral surface in man are not superadded to those found in the higher primates when adult, but to cerebral features exhibited by them when still in the fœtal stage.

This is a relation which is perfectly familiar and well recognized in the case of other organs, but it has never before been thus demonstrated for the cerebrum.

With the data gathered in this chapter, including many tables of measurement new and old, the author next attacks the peculiarities of the cerebral hemispheres according to sex. The use of fœtal material brings out the fact that, in the case of the female, the seventh to eighth month of fœtal life represents a period of accelerated growth for the cerebral fissures. Some fissural variations occur which are characteristic for sex. The definite marks are slight, however; and even in the case of the historic question of the relative development of the frontal lobes, the evidence is still inconclusive. As regards race differences, there is little to be said, since the only way in which peculiarities of fissuration according to race can be given a value is by estimating the relative frequency of their occurrence; and at present the material available from non-European races is too small to warrant a statistical use of it. M. suggests that the brain in the inferior races is wont to show with unusual frequency variations indicating arrested development; but even if unduly frequent, such arrest is certainly slight. When the brains of eminent men are compared with those of persons with average intelligence, the former are found to be more amply developed. The third frontal gyrus contributes to this difference, and the variations are in this locality particularly noticeable. It is to be remarked, however, that just this gyrus is one of the last to be completed, and any differences in growth would therefore be most easy to detect; whereas changes of the same kind occurring at other localities might be more readily overlooked. In cases where the head has been deformed, there are but few records concerning the brain surface. The studies of Ambialet on the '*Déformation toulousaine*' being by far the most important. In such instances the effect of deformation is both general, causing an incompleteness in the later formation of the gyri and local, as indicated by the fact that, along the line of the constricting band, the arrest is most marked. Most instructive is the author's chapter on the criminal brain, since the data for comparison, in the form of well-constructed tables of measurements, are very full. When thus examined the criminal brain exhibits no features which are typical. Among the brains of criminals are to be found some which show an arrest of fissural development, but more than this cannot be said. It may be added that the theory of confluent sulci and theromorphic fissuration are again left without support; and the contention of Benedikt, that such were

the characters of the criminal brain, is as completely refuted by this last study as it already has been by the previous studies of Giacomini, Eberstaller and Cunningham.

In reviewing the condition of the hemispheres in the insane, the deaf-mutes and the microcephalics, the author brings together, especially under the last head, a large number of cases. In all these groups errors of development are the only recognizable features, and these affect principally the later stages of growth. The microcephalics exhibit the most diverse forms of fissuration, the value of which may, so far as the brain is concerned, be estimated on the basis of the comparative embryology of the primate brain, but the phylogenetic value, in a broad sense, will depend on the anomalies of the other systems correlated with them. Pithecoid characters having an atavistic value are rare in idiots, rarer in the insane, and in criminals have never been described; whereas those variations which indicate an arrest of development are frequent among the insane, more frequent among idiots, and most frequent among the microcephalics. As to the cause of microcephalism and the brain-markings characteristic of it, these are to be referred to some pathological condition which prevented provision for the later periods of growth, during the early history of individual development.

H. H. D.

Les localisations cérébrales en psychologie. G. Hirth. Paris, Alcan, 1895.

The author informs us that his aim in this book is to show the possibility of applying the theory of localization to the problems of psychology. We do not believe that he has succeeded in his end; for, in spite of certain ingenious ideas which one comes across here and there in the reading, one closes the book with no clear idea of his theory. As regards the assistance which psychology is to get from physiology, there is no need of exaggerating it. It was the fashion some years ago, in the midst of the reaction against metaphysics, to treat psychology along with physiology; but we are aware to-day that that contest is over, and that experimental psychology has acquired its independence. We are able, therefore, to add that the theories of cerebral localization, which are still hypothetical, do not interest psychology in any direct or fundamental way. Properly controlled, introspective researches afford us much more profitable avenues into the heart of our study. A. B.

Hibernation and Allied States in Animals. Wesley Mills. Trans. Roy. Soc. Canada; Section IV, 1892. Pp. 49–66.

Dr. Mills reviews the evidence regarding the nature and duration of hibernation in the woodchuck, squirrel, black bear, bat, and various kinds of fish. Passing to the discussion of similar states in man, he instances three cases, one of which came under his personal observation, of individuals who remained for long periods in the trance state. The case referred to is described at some length; the patient, an old woman, had remained in a comatose condition for thirteen years; the autopsy showed the absence of any gross lesion of the brain.— Any explanation of the nature of hibernation must take account of the fact that all degrees of functional cessation are found, from normal sleep to the lowest degree of activity consistent with the maintenance of life. There is abundant reason to connect these states together. In sleep the functional activities are lowered; the animal, therefore, requires much less food, and its excretions are greatly diminished. The same laws apply to trance and hibernation. The hibernation of amphibia, reptilia, etc., is protective, being the result of adaptation to environment; this explains equally well why the trait may be lost when the animal is transferred to a different environment. H. C. Warren.

VISION.

Über die latente Hypermetropie. Cl. du Bois-Reymond. Zeitschr. f. Psych., VIII, 34–43. 1894.

When a hypermetropic person makes use of glasses for the first time, he is at first unable to accommodate sharply for the furthest portion of his new accommodation-field; but after short use he gains control of a portion of this, differing in extent with his age. The author attempts to explain this phenomenon. By extending Donder's presbyopic curve into the first years of childhood, he finds a value of about twenty diopters for the earliest accommodation. Dividing the total activity of the ciliary muscle into twenty parts, he calls each of these units an entony (Entonie), and assumes that these units are approximately equal. He further assumes that presbyopia is dependent only on diminished elasticity of the lens, and not at all on a weakening of the ciliary muscle. In case a child is hypermetropic, it fails to gain practice in making use of the first entonies, in number corresponding to the diopters, which are unused; the whole hypermetropia is latent, and a practical far-point of accommodation is formed. If in later life glasses are used, these

first entonies cannot be accurately controlled; but the number of diopters to which they correspond (the latent hypermetropia) diminishes with age, since with diminishing elasticity of the lens the available diopters decrease while the entonies remain constant. We may then formulate the following theory: The practical far-point of accommodation, corresponding to the latent hypermetropia of the first year of life, remains constant for the muscular activity (*i. e.*, measured in entonies) during the whole life, and forms the limit between the manifest and the latent hypermetropia, provided no glasses have been used and the refraction has not changed. The latent hypermetropia, diminishing with age, bears a proportion to the total hypermetropia expressed by the ratio A:20, where A is the accommodation, measured in diopters, belonging to each age.

The author shows that, although the theory is hypothetical, it yet explains all the facts and accords with the actual measurements of latent hypermetropia which have been made by Hirschberg and Daniel.

On the Relation of Accommodation and Convergence to our Sense of Depth. E. T. Dixon. Mind, N. S., IV, 195–212. 1895.

Dixon repeats and extends the experiments, and criticises the conclusions, announced by Hillebrand in an article of which an abstract is given in this Review, Vol. I, p. 540. H.'s experiments led him to the belief that when all aids except convergence and accommodation are excluded, we cannot judge distances monocularly; and that impressions of distance are therefore not produced by movements either of accommodation or of convergence. Dixon used the same methods as did H. for excluding 'empirical' factors, with some improvements. His results are also largely the same. When the object of fixation was moved to or from the observer while he was watching it, there was no accuracy in judging the direction of movement (first series of experiments). When the object was rapidly moved out of the field of view, and another at a different distance was substituted for it (second series), Dixon found: (1) that each of his three observers was able in some degree to judge distances monocularly, but the power of doing so varied greatly in the three cases; (2) that the judgment was directly or indirectly based on the different accommodation required for different distances; (3) that the actual criterion seemed in all three cases a difference in the rapidity or ease with which the accommodation adjusted itself or was adjusted by the observer, and not (as H. explains these results) in any conscious direction of the accommodation by the

observer; (4) all three observers commenced by interpreting the criterion in the right way (showing its association with the impression of depth, at least unconsciously, in ordinary life), though later they differed; (5) Wundt's observation that changes from far to near are more easily observed than changes from near to far, is not confirmed in every case; (6) an absence or defect of accommodating power may enable a person to judge distance monocularly, but this is not the way people with normal eyes judge.

Hillebrand's conclusion is based entirely on the negative results of his first series of experiments. But Dixon shows that the association between convergence and accommodation is a loose one,—in his own case he found that even at 100 cm. distance he could make a difference of over six diopters in convergence without affecting accommodation; that if, in judging distances monocularly, convergence does not actually follow accommodation, it might be that wrong answers were given in just those cases where the convergence went wrong; he found, however, no method of observing with certainty how far this was true; and that the results might also obviously be attributed, not to absence of, but to too gradual change in, the feelings of accommodation and convergence. As to the second set of experiments, the fact that H. "was able to give another explanation, however plausible, of the successes, cannot be held to prove that sensations of convergence are non-existent;" and the facts under (3) above do not uphold H.'s explanation. These experiments do not, therefore, prove, as H. thought they did, that muscular sensations are of no aid in our judgments of depth.

Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten, und ihrer Combinationen. J. Cohn. Philos. Stud., X, 562–603. 1894.

Cohn investigated on fourteen persons the relative agreeableness of different colors and of different combinations of color. His results were: (1) Of two shades of the same color, the more saturated is the more pleasing. Among different colors of approximately the same saturation, the preference appears to depend on individual tendencies, with the exception that yellow is for most observers less pleasing than the others. (2) In case of equally pleasing components, a combination of two colors is the more pleasing the greater difference there is between the components. (3) Two colorless brightnesses are the more pleasing in combination the greater their difference. In comparing single brightnesses, white pleases more than gray or black. (4) If we combine a color with a varying color-

less brightness, or two colors of which one varies in brightness, there is a preference, though less clearly marked, for the greater difference in brightness.

Even in cultivated people, therefore, accustomed to quiet colors and to slight color differences, there exists a preference for saturated colors and for strongly-contrasting combinations, when there is question only of the purely sensory effect, without other considerations. A great many secondary influences, variable and difficult to determine in full, affect the judgment, but their disturbing effect can be overcome or diminished by a sufficiently large number of experiments. These preferences are to be explained, not by any analogy with musical harmony, nor by the effect of simultaneous contrast, but by the fact that differences of sensation as such produce pleasurable effects, entirely apart from the modification of the sensations by one another. E. B. DELABARRE.

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CONSCIOUSNESS AND IMAGINATION.

Imagination in Dreams and their Study. F. GREENWOOD. London, John Lane; New York, Macmillan & Co., 1894. Pp. ix + 198. Price, $1.75.

The author's modest purpose 'being only to convey gossiping hints and suggestions to the generality,' a minute examination of his book would be ungracious; but it must be said that, as a book for the generality, it has serious shortcomings. With the main thesis that dreams deserve a more thorough and rational study than they have yet for the most part had, no fault can be found; the author's criticisms are often just, and some of his suggestions illuminating; but his psychology is antiquated—often more like that of Bunyan's Mansoul than that of the present; and too prominent a place is given to dreams 'which have all the character of prophecy and revelation.' It is to be regretted that one so well equipped in other directions, as Mr. Greenwood seems to be, should not have had the psychological training that would have enabled him to make a really valuable contribution to the scientific study of dreams, which he so ably advocates. EDMUND C. SANFORD.

CLARK UNIVERSITY.

Mémoire et Imagination (peintres, musiciens, poètes et orateurs). LUCIEN ARRÉAT. Paris, Alcan, 1894. Pp. VII + 171.

As the author himself tells us, this is only a preparatory study,

an introduction to the more precise investigation, by means of laboratory and personal methods, which the topic demands. But even with such modest pretentions the book does good service, for it presents many subtle and ingenious ideas. The first part, devoted to memory, includes musicians, poets and orators. M. Arréat examines them in turn on motor, visual, auditory, emotional, and intellectual memory.

It is impossible to sum up briefly so many facts, but we may note in passing that Victor Hugo had enormous command of verbs, but no exceptional stock of qualifying adjectives of a visual kind.

The second part, devoted to imagination, appears to be quite as interesting as the first, and newer. He includes under imagination the facts of manual skill, the movement-memories of the fingers of the designer, the rich verbal flow of the orator, the incidents of his piece in the case of the dramatist. Then he describes the creation of a work of art (p. 127): 1. The conception, whether conscious or not, finished or not; 2. The execution, which is not alone the development of the thought: it reacts upon the thought and modifies it; 3. The emotional state which accompanies the execution; 4. The critical judgment by which the author accepts or rejects what comes to him; 5. A certain doubling of consciousness which results in two different attitudes of mind. Finally, the author draws an interesting analogy between imagination and delirium, and shows that the difference resides in the evident control by the intelligence in the former. A. BINET.

In what Sense are Psychical States Extended? F. H. BRADLEY. Mind, N. S., IV, 225–235. April, 1895.

In a previous article Mr. Bradley discussed the intensity of mental states, maintaining that they possessed degree in various respects, and were in principle, if not in actual practice, measurable quantities. Incidentally he asserted that they, or at least some of them, possessed extensive quantity as well (see this REVIEW, II, 319). To this last point he now recurs. The general principle is that ultimately everything is psychical; but for the purposes of the discussion the less sweeping form of statement is adopted—that any feature of content which makes the meaning of an idea must be present psychically. It is on this principle that mental states are said to have weight and odor, to be long and broad. In what sense, then, is the extension which is predicated of a physical object also in the soul? It is true the physical extended is not present psychically in its full process, and there may even be doubts as to the

extent to which we possess the abstract feature of its extension; still, up to a certain point at least, the extension which we have psychically present is the same as that which qualifies nature. The objection that if psychical states are extended they will collide with one another or with other extended things, is groundless. Spatial things need not be related spatially at all. The soul contains many disparate extensions. Is, then, the soul extended? In respect of certain of its states, the answer must be as given; but in itself, and as a whole, the answer is emphatically, No. For extension is for it not an all-pervasive, but only a particular and subordinate quality; whereas in the physical world it is taken as primary and predominant.

By way of appendix, Mr. Bradley formulates his doubts respecting the 'extensity' regarded by James and Ward as lying at the basis of our perceptions of space. These writers, he says, unite in what are perhaps two errors: claiming to observe extensity as a fact and denying in effect all non-extensive volume. Volume, he finds, indeed, everywhere, but either as implying space outright or as involving something less than extensity. By extensity he apparently understands the quality of 'side-by-sideness.' Nevertheless, while declining to identify volume with extensity, he goes on to mention two aspects of volume—viz., 'its intensiveness and its extent' as present and given, but not distinguished and developed, even in *mere* volume. But is not this undistinguished and undeveloped aspect of extent present, as Mr. Bradley implies, in all sensations, precisely what Ward, wrongly, perhaps, denying plurality to the intensive aspect, means by that 'latent or merged plurality' by which he defines 'extensity?' Whether extensity, in this sense, can be now observed is a separate question. Mr. Bradley admits that some such quality must be postulated.

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EXPERIMENTAL.

Ueber die Beeinflussung einfacher psychischer Vorgänge durch körperliche und geistige Arbeit. Siegfried Bettmann. Psychol. Arbeiten, I, 152–208. 1895.

Herr Bettmann's paper is a study of the mental fatigue effect of physical and psychical work. To secure uniform conditions, the experiments were all made immediately after awaking, the duration of sleep being carefully regulated. The subject (the author himself) arranged his mode of life methodically, avoiding any excite-

ment or labor outside of that required for the tests, and abstaining from tea, coffee and stimulants during the entire period covered by the experiments. The mental labor consisted in adding columns of figures for an hour; the physical labor chosen was to walk rapidly for two hours. The two tasks were performed on different days; at the close of the exercises a series of Choice Reactions were made. To furnish material for comparison, a reaction series was made on other days at the same hour without preliminary exercise. The subject had already reduced his reaction-time by weeks of preliminary practice. The results show: (1) for normal days (*i. e.*, without exercise) a slight reduction of the average time during the first sixty reactions, then a gradual increase to about 300 σ at the end of 300 reactions; (2) for the days of mental exercise, a very constant average of about 380 σ during the entire series of 300 reactions; and (3) for days of bodily exercise, a steady *decrease* of the times from about 300 σ to 230 σ—some of the figures running as low as 190 σ. On the other hand, the last case shows about 25 per cent. of anticipations to 1 per cent. for the tests following mental exercise, and 2.5 per cent. for normal days.

The results indicate that mental impairment follows from physical as well as from psychical labor; in the latter case the reaction-times are shorter, but the enormous number of anticipations neutralize any gain from this source. Some further experiments, in which the tests were word-reactions and the facility of learning twelve-place numbers respectively, serve to accentuate this effect: as the difficulty of the tests was increased, the apparent advantage of the physical exercise was rapidly lost.

The author criticises the results of Mosso and Vintschgau, on the ground that their conditions of experimentation were too complex to admit of discrimination between various affecting influences; in his own experiments the conditions were as simple and uniform as it seems possible to make them. H. C. WARREN.

Minor Studies from the Psychological Laboratory of Clark University: under the direction of E. C. SANDFORD. VIII. *A Study of Individual Psychology*, CAROLINE MILES. IX. *The Memory After-Image and Attention*, A. H. DANIELS. X. *On the Least Observable Interval between Stimuli Addressed to Disparate Senses, and to Different Organs of the same Sense.* XI. *Notes on New Apparatus*, E. C. SANDFORD. Am. Jour. Psych., VI, 534–584. Jan., 1895.

VIII. The psychologist cannot hope to solve all his problems by

experiments in the laboratory. Some questions can be solved only by introspection. If, however, introspective results are to be of value, they must be confirmed by others. The method advocated by Miss Miles is to make a list of such introspective questions, and obtaining answers from a large number of people, to secure thus a vast amount of valuable data. Of course there are difficulties. The answers of untrained observers are naive and unreliable. Many subjects are unconsciously influenced either by aversion to revealing an undesirable phase of their inner life or by the desire to give results satisfactory to the experimenter. Notwithstanding these difficulties the method is valuable; more, however, for its suggestiveness than for the actual results obtained.

The following headings will indicate the character of the questions asked of 100 Wellesley women: I. Habits of discrimination and memory; II. Methods of concentrating attention and of getting asleep; III. Emotions and preferences; IV. Recollections of childhood; V. Miscellaneous.

IX. These experiments (as those of XI) are interesting from their bearing upon the attention. This study aims to estimate the duration of memory after-images—a phenomenon familiar to all psychologists. The memory after-image is differentiated from memory by association—by distracting the attention as far as possible from the sensation when received. A group of three digits was pronounced while the attention was distracted by the reading aloud of interesting stories, and the subject was required after a given interval to reproduce them. The intervals were 0, 5, 10, 15 and 20 sec.

It was found that the longer the interval the less probability there is that the reproduction will be correct. Less than 15 sec. is the limit; beyond this the images could not be reproduced. If perfect distraction could be attained, this limit could be decreased almost indefinitely. If, however, the digits return to consciousness before the given interval has elapsed—to which, as it is important to note, there is a great tendency—the limit is considerably extended (perhaps to 20 sec.) and greater exactness is attained. As we should expect, there are more errors with the first and second digits than with the third. The article concludes with a comparison of these experiments with those of Dietze and Wolfe.

X. "The object of the following experiments was a remeasurement of this interval for single pairs of stimuli under varying conditions of the attention." In the first series, with unforced attention, an important result is the unanimity of the requirement of a longer interval for the 'click-flash' combination than for the 'flash-click.'

This is interesting, as it is 'flatly opposed' to the results of Exner in all his seven subjects. Exner's data being false, his explanation that the F.-C. requires a longer interval, because of 'the slower rise and greater duration of the visual sensation,' is, of course, invalid. The author favors the explanation of the differences in the intervals by the movements of the attention (Wundt) rather than by 'sensory inertia.'

In the second series, with forced attention, it was found, contrary to expectation, that voluntarily attending to a specific stimulus has no appreciable tendency to make it appear to precede the other. The best results were obtained, not when the attention was preadjusted, but when the subject was in a state of 'alert indifference.' An explanation of this preadjusted attention is suggested. We decide upon the order of the occurrence of stimuli, not by the order of the sensations resulting thereby, but by the order of the sensations from the reflex movements in the adjustment of the organs in response to the stimuli. If the attention be preadjusted, the sense-organs are more or less adapted, and consequently the reflex movements being more obscure, the judgment is correspondingly uncertain.

XI. Four new pieces of apparatus are here described. The Binocular Stroboscope is an instrument devised by Prof. Sandford for demonstrating a 'little-known' phenomenon of binocular vision —viz., the definite localization of a moving object due to an instantaneous glimpse by each eye in succession. There are also described, 'A Model of the Field of Regard;' 'A Simple Adjustable Stand,' which can be put to a variety of uses; and 'The Pendulum Circuit Breaker,' an instrument "designed to break electrical currents at known and regulable intervals of time from one another." This last instrument was used in the experiments of Study X to obtain an exact interval between the application of the different stimuli.

W. J. SHAW.

Minor Studies from the Psychological Laboratory of Cornell University. Communicated by E. B. TITCHENER. VI. *Taste Dreams.* E. B. TITCHENER. VII. *On the Quantitative Determination of an Optical Illusion.* R. WATANABE. VIII. *The Cutaneous Estimation of Open and Filled Space.* C. S. PARRISH. Amer. Jour. of Psy., VI, 505–523. Jan., 1895.

VI. This study aims to show that the explanation of dreams, adduced in connection with the other sensations, is adequate for gustatory dreams also. Dreams are of two kinds: presentative

(occasioned by a peripheral stimulus) and representative (due to central excitation). The tendency of psychology is to emphasize more and more the relative importance of the peripheral stimulus; thus the *Eigenlicht* of the retina explains the great predominance of visual dreams.

The number of taste dreams is relatively small. Miss Calkins (A. J. of Ps., V) reports only two in a total of 335 dreams, and none in a total of 298. While without doubt the majority of dreams of the gustatory type are representative, we have no right to assume that they are all presentative any more than we have to assume that all visual dreams are presentative, because the retina is in continuous excitation. In conclusive support of this, five taste dreams were collected, all representative. Two of these are described—one a case of auto-suggestion, being experienced by the author himself.

VII. This article is a continuation of an investigation of the illusion of point and dotted line distance begun by Mr. Knox (A. J. of P., VI, 3). The apparatus consisted of a set of cards, on each of which was placed a line, made up of a dotted line, and a point distance, the centre of the line coinciding with the centre of the card. Knox, by keeping the point-distance constant, and varying the dotted line until the subject judged them equal, found, besides confirming the illusion itself, that the mean variation of the vertical differences is not greater than that of the horizontal. This result was unexpected, because "binocular bisection of horizontal distances is not subject to any constant error," while of vertical lines we constantly over-estimate the upper part.

Dr. Watanabe, 'in the hope of elucidating this difficulty,' conducted a similar series, except that for him the dotted line is the constant. His results on three subjects confirm those of Knox, in showing not only that the illusion itself prevails in all cases, but also that the vertical differences have not a greater mean variation than those of the horizontal. In regard to this latter, he says: "The fact itself holds, [but] we are not at present prepared to suggest any explanation."

VIII. It has been amply proven by experiment that, to the eye, filled space invariably appears greater than empty space, objectively equal. This has been explained by the fact that the movements of the eye are more retarded in the case of filled space. Dresslar (A. J. of Ps.) maintains that the same illusion holds in the case of touch, and the same explanation—viz., movement plus touch—is adequate. Prof. Parrish maintains that, so far from Dresslar's explanation

being adequate, the alleged facts on which it is based are not true—*i. e.*, filled space is not judged by touch as greater than empty space. On the contrary, he shows by two series of experiments upon seven reagents that filled space is judged actually shorter.

In the first series, nine pieces of wood were used, to each of which was attached a number (varying from two to nine) of small rubber projections, the distance between the extremes being constant—64 mm. These were applied to wrist and forearm. The result shows "that for a resting skin a filled distance is, on the average, shorter than an empty distance objectively equal to it."

In the second series, the apparatus consisted of a strip of rubber .01 in. in thickness (for filled space) and an æsthesiometer (for empty space). The results here confirm the first; filled space is invariably judged shorter than open space—*e. g.*, "an open space of 24 mm. is judged equal to a filled line of 28 mm., and one of 15 to a filled line of 20." The author concludes that these experiments upon resting skin, showing an illusion opposite to that of sight, clearly support the explanation of the latter in terms of movement.

WESLEYAN UNIVERSITY. W. J. SHAW.

Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Schalleindrücken. Ueber den Einfluss der Geschwindigkeit des Pulses auf die Zeitdauer der Reactionszeit bei Licht und Tasteindrücken.—J. J. VAN BIERVLIET, (Phil. Stud., X, pp. 160 ff; XI, 1, pp. 125–135.

In these two papers are communicated the results of the first part of an investigation undertaken by Professor van Biervliet in the psychological laboratory at Ghent to determine the influence of the circulation of the blood upon the length of the reaction time. The present communications report upon modifications in the duration of the simple reaction to stimuli of sound, light and touch, accompanying varying rates of pulse beat. Further investigation will concern itself with the effects of changes in blood pressure.

In all, 17 different observers took part and the results of 6800 experiments are reported. The subjects were given preliminary practice. The experiments were made in series of 20; before each series, the pulse rate at the wrist was determined by two independent observations. The ordinary set of apparatus was employed, including the Hipp chronoscope, a hammer to give the sound stimulus, the Leipzig pendulum for the light stimulus, and for touch a hammer, which was allowed to drop upon the first joint of the middle finger. The subject sat in a dark room apart. In the

sound series, each subject made 400 reactions in all; in the light and touch series, results are collated from 200 '*useable*' reactions of each individual. The reactions are designated as *sensory*. The bell signal was given 5 seconds before the stimulus in each experiment. In the sound series the stimuli followed one another at one minute intervals; in light and touch, at 15 seconds.

Typical results are the following: In the sound series the reaction time of one subject diminished from 130 to 117 as the pulse rate increased from 70 to 110 beats per minute; a second subject decreased in reaction-time from 144 to 130 with a pulse rate increasing from 70 to 100; a third from 154 to 130 with pulse rate increasing from 70 to 100. With the light stimulus, a pulse rate increasing from 60 to 80 gave a reaction-time decreasing from 187 to 174; one of 60 to 110 gave reaction-time of 208 to 123; a third increasing from 80 to 100 gave decreasing reactions of 209 to 176. With the touch stimulus, pulse rate increase from 60 to 100, reaction-time decrease from 209 to 176; pulse rate increase 70 to 90, reaction-time decrease 186 to 150; pulse rate increase 80 to 100, reaction-time decrease 167 to 164; pulse rate 80 to 100, reaction-time 175 to 164.

From these results, Professor van Biervliet concludes that as a rule, the time of a reaction diminishes with an increase in the pulse rate. He finds however that with the pulse at a minimum and especially at a maximum, the variations in the reaction-time do not follow this rule. Differences in pulse rate, therefore, may produce remarkable changes in the duration of the reaction-time. In all investigations into the phenomena of attention in which the length of the reaction-time is used as a means of analysis, it would seem necessary in consequence to give some consideration to the condition of the pulse.

These conclusions of Professor Biervliet are not justified by his tabulated results. The average results of the individual series do follow his theory closely, but the large mean variations in conjunction with the small differences in the averages of reaction-times, make the experiments entirely negative. In nearly every case the mean variation of the single experiments from the average results of the series is larger than the maximum difference in reaction-time for the extremes of pulse rate. A pulse of about 70 beats per second gave an average reaction-time of 130, while one of 110 beats gave 117. This result may or may not be significant of real difference in reaction-time, depending upon the number of experiments composing the average result and the size of the mean variation of the individual experiments from this result. In the case taken, the

mean variation was about 30. This result only signifies that with a pulse of 70 beats, the reaction-time lies between 100 and 160, while with one of 110 beats it lies between 87 and 147. There is no experimental evidence in the result to show that with a pulse beat of 70 the reaction-time may not be 100 and with one of 110 it may not be 147. The difference between a reaction-time of 167 and 164, both averages obtained with mean variations of 16, and that between 175 and 164 averages with mean variations of 18, are purely accidental. To prove that results differing so slightly are significant of a real difference, the mean variation must not be greater than 1 or 2 and the experiments must be more than a few score in number.

In the interests of psychometry, it may be well to call attention to the importance of giving the mean variation of the recording instruments. It is very probable, that a large part of the variation in the results is due to the variable error of the apparatus. This ought to be greatly reduced. The number of practice experiments ought to be given, and the results of practice series ought not to be rejected from the final showing. It is desirable to know what is the nature of the practice in which the *subject learns to react* and what is the effect of that practice upon the result. Further, the value of all results would be increased if it were reported how many and under what conditions results were excluded "*because they varied too much from the average*".

University of Pennsylvania. Lightner Witmer.

MOVEMENT.

L'Écriture et le caractère. J. Crépieux-Jamin. Paris, Alcan, 1895. Pp. 442.

Although there is no such science as graphology, and no such men as graphologists, and although it might well be asked what interest attaches to the study of handwriting, nevertheless it is certain that the study itself is attractive and useful. Handwriting involves the most complicated and delicate of acquired movements: it is comparable in this respect with the movements necessary to hold and play an instrument; besides, as its purpose is the translation of thought and emotion, it possesses an expressive function, just as mimicry and gesture do. These two reasons appear to me to be more than sufficient to make desirable the profound study of handwriting. But a second question arises; that of method. It is evident that a simple method is necessary here as elsewhere; that of observation, first of all, of writing as a kind of movement, with an

analysis of the different elements of movement which enter into it; taking account of the pressure, rapidity, etc., with description of the form. There are psycho-physical instruments which might be used for this analysis. Then, following this study of normal writing, that of its various modifications, especially the influence of age. What are the principal marks of handwriting from the first pothooks of the child? What are the modifications due to old age? Are there family characteristics? Can we detect any hereditary influence apart from the influence of imitation? Then would come the disturbances of handwriting: the influence of cold, fatigue, alcohol, and various poisons, disturbances due to disease, etc. In short, handwriting should be put to the same series of tests that all physiological facts are put to.

It must be confessed that psychologists have rarely followed such a truly scientific method. And although M. Crépieux-Jamin is undoubtedly one of the most serious and exact of graphologists, he yet makes no use of the method of observation and experiment, except incidentally. Indeed, research in this subject seems to have been badly oriented from the first. The most complex and difficult questions have been put, such as the reading of the writer's character in his handwriting. We have all had curiosity in regard to letters, and have tried to attach to the writing some information about the writer; but to understand the difficulty of it, one has only to call to mind what a complex thing we mean by character. There is nothing more complex, more changing, more vague. So how can we attack such a problem? The early graphologists did not state their method, if, indeed, they had any. They took the attitude of those who found it necessary to keep a monopoly. Their successors, especially Lavater and the Abbé Michon, did make their method known; and it may be described in two words—intuitive and analytic. The first describes a sort of subconscious divination founded on long experience. The second process discovered a certain number of signs in the writing, each of which is capable of direct interpretation. M. Crépieux-Jamin adopts this second method, although making a certain use also of intuition. He reaches the most minute rules of procedure. In order to reach good results, he says it is necessary to have a large number of writer's specimens; while the writing on official documents is worthless, that of private letters is most valuable. There are three stages in the interpretation of the signs: first, general signs, such as clearness, angle of the letters, etc. Then come particular signs, such as the identity of 'n' and 'u,' that of the way 't' is crossed, etc. And, finally, in

order to complete the psychological portrait of the writer, one must study the evolution involved—a very delicate task—which consists of a judgment of the effect produced upon the writing by the addition of one mental quality to another. . . . Some of the author's analyses under this last head are very fine, and this part of his book is very pleasant reading. But from the point of view of science, the newest portion of his book is useless—that which deals with handwriting under irregular conditions. In it he finds thirty-nine different signs, each of which has a special psychological significance.

All this does not constitute science; it is simply exposition. And the tone of authority which may be put into an affirmation does not increase its demonstrative force. The feeblest mar of the bookk is lack of proof. . . If we should attempt to show what the author has not done in this respect, we should begin by citing the proposition that writing is a kind of gesture or a series of small gestures; and that the relation of writing to character can be made out only on the basis of this analogy with gesture (p. 37); so far we go with him. But he then establishes a series of presumptions on the probable value of writing. . . And in it all, as in his treatment of graphic signs, he gives only what is vague and ambiguous.

Yet he has collected in his useless chapter on 'Experimental Graphology' some interesting experiments made upon himself. He tried writing five or six lines under varying conditions, such as heat, cold, fatigue, joy, and sorrow, etc. The idea is excellent, but it is to be regretted that the experiments were not repeated on persons who did not have preconceived ideas; and that these experiments, which are difficult to get, were not made the real basis of his book. Such experiments, of course, would not give the key to character; to study that question, hypnotism might be called in as a method. We might give to proper hypnotic subjects suggestions of simple emotions; see that they realize them; and then set them to writing under the influence of these emotions. Then we might compare different specimens of such writing. As I said ten years ago, that would be the best method of experimental graphology.

SORBONNE, PARIS. A. B.

ETHICAL

The Study of Ethics. A Syllabus. J. DEWEY. Ann Arbor, Mich.: Register Publishing Co. The Inland Press, 1894. Pp. 151.

This study has as its aim "to justify the belief that amid the prevalence of pathological and moralistic ethics there is room for a theory which conceives of conduct as the normal and free living of

life as it is." It attempts a psychological analysis of 'active experience' and a revelation through this analysis of the 'chief ethical types'. Those who have found the author's former work 'Outlines of Ethics' stimulating, suggestive and original will find here the same characteristics, combined with a genuine philosophic candor and downright manliness of expression. In form and method it is a model of what a syllabus should be. It is a pity, however, that anything so good should be marred by such an absurdly large number of typographical errors.

The contents are divided into nine chapters treating successively of (1) the nature of ethical theory, (2) the factors of moral conduct (3) a general analysis, (4) the moral consciousness, (5) moral approbation, (6) reflective approbation, conscience, (7) nature of obligation, (8) freedom and responsibility, (9) virtue and the virtues. In these chapters we are introduced again to some doctrines already familiar to readers of Professor Dewey's articles in the philosophical reviews, *e. g.*, to the doctrine of the mediation of impulse (will)—"the expression of every impulse stimulates other experiences and these react into the original impulse and modify it. The reaction of the induced experiences into the inducing impulse is the psychological basis of moral conduct. . . . . This back reference of an experience to the impulse which induces it we may term the mediation of impulse"; to the doctrine of the agent's moral situation as "nothing but the complete co-ordination of all his powers (abilities) and relations. . . . The good man in a word is his whole self in each of his acts, the bad man is a partial (and hence a different) self in his conduct."

The critical examination of the hedonistic and Kantian ethics is searching and convincing, both theories being condemned for their abstractness. Green's theory of the moral ideal is also made the subject of an illuminating criticism.

Every student of ethics should read this book. To the reviewer's mind it is one of the most suggestive published in recent years.

MIAMI UNIVERSITY. R. B. JOHNSON.

NEW BOOKS.

Saggi di Psicologia del Bambino. P. LOMBROSO. Turin and Rome, L. Roux, 1895. Pp. XII + 284.

Introduction to Social Philosophy. J. S. MACKENSIE. 2nd ed. London and New York, Macmillans, 1895. Pp. XIV + 454.

The Pathology of Mind. H. MAUDSLEY. London and New York, Macmillans, 1895. Pp. XI + 571.

Philosophie und Erkenntnistheorie. Ite Abth. L. Busse. Leipzig, Hirzel, 1895. Pp. VI + 289.

Éléments de psychologie humaine. J. Van Biervliet. Paris, Alcan, 1895. Fr. 8.

Ueber das Grundprincip der Association. Allin. Berlin, Mayer and Müller, 1895.

Gewöhnung und Gewohnheit. (Dissertation, Jena.) A. P. Kurtidis. Athens, Konstantinidis, 1895. Pp. 64.

Die Aufgaben und Methoden der Psychologie in der Gegenwart. J. Merkel. Zittau, Böhme, 1895. 4to, pp. 36.

Vorlesungen über soziale Ethik. G. v. Gizycki. Berlin, Dümler, 1895. Pp. 88.

Zur Psychologie des Schreibens. W. Preyer. Hamburg u. Leipzig, Voss, 1895. Pp. 230.

Die Grundprobleme der Philosophie. I. Socoliu. Bern, Beck-Keller, 1895. Pp. XVI + VI + 260.

Princeton Contributions to Psychology. Vol. I, No. 1. Reprinted from Psych. Rev. Edited by J. Mark Baldwin. Princeton University Press, 1895. Pp. 44.

Die Zweckmässigkeit der psychische Vorgänge als Wirkung der Vorstellungshemmung. O. Lindenberg. Berlin, Duncker, 1894. Pp. 64.

The Essentials of Logic. B. Bosanquet. New York, Macmillan, 1895. Pp. X + 168. $1.

Transactions of the American Medico-Psycholog. Association. Vol. I. 1894 (Semi-Centenn. Proceedings). H. M. Hurd, Sec. The Assoc., Utica, N. Y., 1895. Pp. X + 361.

NOTES.

Prof. W. L. Bryan is now Vice-President of the University of Indiana and Professor of Philosophy; Dr. John A. Bergström is Assistant Professor of Psychology and Pedagogy, and Mr. E. H. Lindley, Instructor in Philosophy, in the same institution.

The death is announced of Prof. Luigi Ferri, of the University of Rome, and of Prof. G. Glogau, of the University of Kiel. An Italian committee solicits subscriptions for a monument to Professor Ferri. American subscribers may send names and amounts to this Review, or to the Editor *Rivista Italiana di Filosofia*, via del Govorno Vecchio, 121, Rome, Italy.

Mr. H. M. Stanley has in the press of Swan Sonnenschein & Co., a work titled *Studies in Evolutionary Psychology.*

Dr. Georg von Gizycki, Associate Professor of philosophy in the University of Berlin, died on March 4 at the age of 46.

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The Psychological Review.

SOME OBSERVATIONS ON THE ANOMALIES OF SELF-CONSCIOUSNESS.[1]

(I.)

BY PROFESSOR JOSIAH ROYCE,
Harvard University.

In the present paper I shall venture to lay some stress upon certain familiar factors whose psychological influence upon the growth and the anomalies of self-consciousness, both in normal and in abnormal human beings, seems to me to have been, from the purely theoretical point of view, rather unduly neglected. In particular, I shall try to indicate how these theoretically neglected factors may help to explain certain well known types of variation, and of abnormality, to which the functions of self-consciousness, as they empirically appear, are subject. Meanwhile I shall of course avoid, in this paper, any positive reference to the distinctively metaphysical problems which the word self-consciousness easily suggests. The philosophical aspects of the problem of self-consciousness belong altogether elsewhere. Starting this evening with the mere empirical fact that any normal man has, as part of his mental equipment, conscious states and functions that involve, in one way or another, his experience, his knowledge, his estimate, or, in general terms, his view, of himself, and remembering that, in many defective and disordered people, these, the functions of individual self-

[1] A paper read before the Medico-Psychological Association of Boston, March 21, 1895.

consciousness, undergo changes of a manifold and interesting sort, I shall try to illustrate aspects of the purely psychological theory of our topic. I speak to practical men, who are also men of science. I need make then no apology for introducing here a problem which, whatever its difficulty, is full both of scientifically attractive, and of practically important elements. For surely the alterations and defects of the functions of self-consciousness are amongst the most frequent phenomena in the region of mental pathology.

I.

In its inner aspects and relations, what we mean by self-consciousness, in any one man, is an enormously complex function or rather a little world of functions. But this world of functions is centred about certain well known habits and experiences which at once serve, not to explain it, but in a measure to begin for us the definition of our problem. There are, namely, in any mature person, certain established motor habits, which, according as they appear to be intact or not, enable us at once to test, from without, the relative normality of whatever belongs to that which one may call the mere routine of an individual's self-consciousness. There are also certain inner experiences, in terms of which the normal individual himself, from moment to moment, can feel assured of the apparent naturalness of his own notion or estimate of himself. A mature man whose self-consciousness is normal, if his means of expressing himself are intact, must be able to explain 'who he is,' *i. e.*, he must be able to tell his name, his business, his general relations in life, and whatever else would be essential to the practical purpose of identifying him. Furthermore, his account of himself must be able to show an estimate by no means adequate or infallible, but at least not too wildly absurd, of his actual degree of social dignity, of his personal importance and of his physical capacity. He will to be sure quite normally estimate his value, his prowess, or even his social rank, not, in general, precisely as his fellows do. But this sort of estimate has its normal, if rather wide, limits of error. If these limits are passed, the man's account of himself proves the presence of a de-

rangement of self-consciousness. Finally, as to this account which the normal man can give of himself, he must show a certain degree of correctness as to what he can tell us of his body and of its present state. Here, of course, the limits of error are very wide, but are still pretty definite. A man is normally a very poor judge of his internal bodily states. But if he says he is made of glass, or that he is aware that he is a mile high, or that he is conscious of having no body at all, we recognize a disorder involving alterations of self-consciousness.

Within his own mind, meanwhile, and from his own point of view, a man normally self-conscious is more or less aware of a great deal about himself of which it is notoriously hard for him to give any exact account whatever. Yet this internally normal self-consciousness has, at any time, a definitive, if not easily definable content, which, in its relatively inexpressible complexity of constitution, far transcends what one expresses when he tells you his name, his place in life, his degree, or his notion of his bodily condition. This normal inner self-consciousness involves, in the first place, what we are now accustomed to call, from a psychological point of view, masses of somewhat vaguely localized bodily sensations, which, just in so far as they affect our general consciousness, are not sharply differentiated from one another. The origin of these sensations lies in the skin, in the muscles, and, in part, in the viscera. Moreover, the visual perception of the body, the auditory experiences of the sound of one's own voice, and yet other sensory contents, including the more general sensations of bodily movement, obviously determine, now more, and now less, the content or the coloring of normal self-consciousness. If any of these masses of sensory contents are suddenly altered, our immediate self-consciousness may be much changed thereby. Dizziness, sensations of oppression in the head, a general sense of bodily ill-being, a flushed face, a ringing in the ears,—any of these may involve what we primarily take to be a general alteration of our feeling of self, and only secondarily distinguish from the self as a separate and localized group of experiences. In general, the more sharply

we localize our sensations, and the more we refer them to external objects, the less do these sensory experiences blend into our total immediate feeling of ourselves. The localized or objectified sensory state appears as something foreign, as coming to us, as besetting us, or as otherwise affecting us, but not as being a part of the self; and only a relatively philosophical reflection regards even our perceptions as part of ourselves. Our more naïve self-consciousness tends to regard the sensory or immediate self as a vague whole, from which one separates one's definite experiences of this place on the skin, of this color or tone, or of this outer object.

Yet our inner notion of the self of self-consciousness is by no means confined to this cruder apperception of massive sensory contents. In addition, our normal mature awareness of who and what we are means what one may call a collection of feelings of inner control, of self-possession, or, as many would say, of spontaneity. If such feelings begin to be altered or lost, one complains of confusion, of a sense of self-estrangement, of helplessness, of deadness, of mental automatism, or of a divided personality. As a fact, since the associative processes always depend upon the conditions of which we are not conscious, our sense that we can and do rule our whole current train of conscious states is, as it is ordinarily felt, a fallacious sense. But if we cannot really predetermine, in consciousness, what idea shall next come to consciousness, but are dependent, even in the clearest thinking, upon the happy support of our associative mechanism, it is still normal to feel as if, on the whole, our inner process were, in certain respects, relatively spontaneous, *i. e.*, as if it were controlled by our ruling interests and by our volition. This sense of inner self-possession is, to be sure, an extremely delicate and unstable affair, and is constantly interfered with, in the most normal life, not only by a series of uncontrollable sensory novelties, due to the external world, but by baffling variations, either in the play of our impulses and ideal associations, or in the tone of our emotions, or in both. Yet, when we are alert, these little interferences continually arise only to be subordinated. We

have perhaps momentary difficulties in recalling names or other needed ideas, of an imperfectly learned group, or we feel equally momentary indecisions as to what it is just now best to do, or we find our attention wandering, or our emotional tone disagreeably insistent, or our impulses numerous and wayward. But in all such cases we can still, in the normal case, 'keep hold of ourselves,' so that we accept as our own whatever content triumphs in the play of associative processes, and find our essential expectations met, from moment to moment, by the inner experiences which form the centre of the mental field of vision. If this rule no longer holds of our inner life, then our self-consciousness begins to vary, and we suffer from confusion or from other forms of the sense of lost inner control.

Thus the self of ordinary self-consciousness appears at once as a relatively stable group of unlocalized sensory contents or contents of feeling, and as the apparent controller of the train of associated ideas, impulses, and acts of attention or of choice. Of course these two aspects of the self are closely related. It is the associative potency of the ruling feelings and interests that most secures the fact and the sense of inner self-control. But meanwhile the self also seems, or may seem, to its possessor, much larger than any group of facts or of functions now present. One notoriously regards the present self as only the representative of a self which has been present, in the remembered past of our lives, and which will be present in the expected future to which we look forward. Nor does self-consciousness usually cease with this view. The characters, attributes, functions, or other organic constituents of the self commonly extend, from our own point of view, decidedly beyond anything that can be directly presented in any series of our isolated inner experiences, however extended. When one is vain, one's self-consciousness involves the notion that one's self really exists, in some way or other, for the thoughts and estimates of others, and is at least worthy, if not the possessor, of their praise or of their envy. When one feels guilty, one does not and cannot abstract from the conceived presence of one's self in and for the experience of a real or ideal judge of one's

guilt. In all such cases the self of self-consciousness thus appears as something that it would not and could not be were there not others in the world to behold, or to estimate it, to be led or otherwise influenced by it, or to appeal to it. It is now from such points of view that the self of self-consciousness comes, in the end, to get form as a being who takes himself to have a social position, an office, a profession,—in brief, a vast group of functions without which the self would appear itself to be, relatively speaking, a mere cipher, while these functions are at once regarded as organically joined to the self, and centered in it, and, nevertheless, are unintelligible unless one goes beyond one's private consciousness, and takes account of the ideas and estimates of other people.

Every normal man thus knows what it means to be a person with a social position, or a dignity, or a place in the world, or a character, a person vain of himself, or ashamed of himself, or socially confident or timid about himself, or otherwise disposed to view himself either as others seem to view him, or as he fancies that they ought to view him, or as he has faith that God views him. And such a view of one's self cannot be satisfied with any group of inner facts, however extensive, as containing within it the whole of one's ego. This view conceives the office, calling, dignity, worth, position, as at once a possession, or a real aspect, of the self, and as a possession or an aspect that would vanish from the world were not the self conceived as existing for others besides itself, in other words, were not the self conceived as having an exterior as well as an interior form of existence.

The self of normal self-consciousness, then, is felt at any moment as this relatively stable group of inner states; it is also felt or conceived as the supposed spontaneous controller of the general or of the principal current of successive conscious states; it is remembered or expected as the past or future self, which is taken to be somehow more or less precisely the same as the present self; and finally, it is viewed as having a curious collection of exterior functions that involve its actual value, potency, prowess, reputation, or office, in its external social relations to other actual or ideal selves, *e. g.*, to its neighbors, to humanity at large, or, in case one's faith extends so far, to God.

And, now, just as the immediate self of the mass of inner sensations and feelings can vary, or just as the self of the sense of self-control can be more or less pathologically altered; so too the identical or persistent self of memory can be confused, divided, or lost, in morbid conditions; and so too finally, the self of the social type of self-consciousness is subject to very familiar forms of diseased variation. The social self above all can come to be the object of a morbidly depressed or exalted inner estimate. One's social prowess, position, office and other relations, both to God and to man, can be conceived in the most extravagantly false fashions. And furthermore, as I wish at once to point out, the most noteworthy alterations of self-consciousness, in insanities involving delusions of suspicion, of persecution and of grandeur, appear upon their very surface as pathological variations of the social aspect of self-consciousness. Note at once the possible significance of this fact. However you explain delusions of guilt, of suspicion, of persecution and of grandeur, however much you refer their source to altered sensory or emotional states, they stand before you, when once they are well developed, as variations of the patient's habits of estimating his relations to other selves. They involve, then, *maladies of the social consciousness*. The theoretical significance of this fact surely seems worthy of a closer consideration than it customarily receives.

Since the psychologist, as such, can afford to be quite indifferent to the question whether any real being, to be called an Ego, exists, or not, and since he is therefore still less interested in the philosophical problem as to the forms of being which a real Ego can possess, in case it exists,—I am here very little concerned to answer one question which these latest considerations may have already suggested to some of you. I mean the question whether an Ego really can possess that equivocal sort of exterior existence, outside of its own train of conscious experience, which, as we have seen, the social sort of self-consciousness seems to attribute to the self. When I feel humble or exalted, abased or proud, guilty or just, or when I say, 'I am in this social office or position,' I seem to myself as one whose actual nature and functions

include more facts than can ever be crowded into my own consciousness. For unless I believe in my real relations to my neighbors or to God, and conceive those relations as somehow a part of myself, I should have no material out of which to weave my notion of my rank, or my duties, and of my external importance. But whether this idea of myself is defensible or not, from a philosophical point of view, is far from us here. It is enough for us that a man commonly has just such a view as to his own nature, and that pathological variations of such a view are familiar and important phenomena.

II.

In the foregoing sketch, I have been simply reporting familiar psychological phenomena. That our human self-consciousness involves all these various elements, is, one may say, agreed. The problem is, how have all these elements come thus to hang together? And so we next have to attack the central problem just mentioned, *i. e.*, we have to ask, in a purely psychological sense: How does this elaborate mental product called self-consciousness get formed out of these numerous elements and why, when once formed, is it so variable, and, finally, why, when it varies, does it vary in the directions so frequently reported?

It is here that our theoretical knowledge is at present so poor. The collection of observed facts is, to be sure, at present, considerable. Readers of Ribot's book on the 'Diseases of Personality,' know of the general types of varying self-consciousness to which attention has been most attracted. Loss of the sense of personality; or again, the delusion that one is dead, or is lost, or is an automaton; or the feeling or idea that there is a foreign or other self within one; or the attribution of one's own thoughts, or acts, to another and wholly external person or persons; or the alternation or the apparently actual multiplication of one's own personality; or the refusal to regard one's present self as identical with one's past self: such are some of the variations to which self-consciousness is subject, in addition to the before-mentioned alterations of

the obviously social type of self-consciousness. But when we ask why any of these alterations takes place, we have so far only one unquestionable, but theoretically inadequate answer, viz.: In all such cases there are alterations of the common sensibility, or of the memory, or of both. Now one sees, without doubt, that self-consciousness involves the common sensibility, in the sense before indicated. One sees then that if this core of normally stable, vaguely localized sensory conditions and feelings gets altered, one's notion of one's self may also naturally change. And, not to leave the limits of ordinary experience, one knows and understands what it means to say, when these central masses of feeling do more or less change: 'I feel queer; I feel altered; I am no longer quite myself; I am not my old self.' By a little stretch of imagination one can also understand such a delusion as 'I am made of glass,' quite as well as one can understand any other delusion. For here our dreams help us to see our way, and we have only to suppose that a certain association of ideas, whereby a partial anæsthesia gets interpreted, becomes fixed, and exclusive, in order to see how the delusions as to bodily condition or constitution, present in a measure in all hypochondriacs, can assume such extreme forms. Just so too the mere assertion 'I am lost,' or 'I am dead,' is, on the face of it, just an insistent verbal statement, or at best an inner judgment whose exclusive presence in consciousness is due merely to morbid habit, and whose meaning or logical consequences we often need not suppose the patient to develop in any delusionally definite form at all. These phenomena involve, where they are alone, or are segregated from the rest of the patient's life, rather pathological simplifications of the contents of consciousness, morbid associations of sensations with simple groups of words or of ideas, than any other processes. So far, then, we see some light.

But now the case is otherwise when one says: 'There are two of me,' and proceeds actively to develop the consequences of this inner variety of self. Here, to be sure, the phenomena of dreams, and of the commoner forms of transient delirium, as in fevers, bring this sort of doubleness

within the remembered experience of very many persons; and familiar moral and poetical statements about the two selves or more that dwell in one's breast, assimilate such experiences to those of normal people. But one's consciousness, in such cases, throws little direct light upon how the phenomena arise. Sometimes, to be sure, in delirium their basis is plainly hallucinatory, as when a fever patient sees himself, in bodily presence, standing at a distance, or lying in the bed. But even then one wishes for more light as to the question whether and how such a tendency to pathological duplication has any natural foundation in the understood habits of normal life. This problem seems even the more insistent when one observes that the sense of the inwardly doubled personality often arises without any obvious basis in hallucinations of the special senses. But in such cases, our present theories often fall back again upon the variations of the common sensibility. Yet here one fails to see how any easily conceivable alteration in the contents of the central core of the sensory self is by itself sufficient to explain a tendency to apperceive that self as double. One does not doubt the existence, in such cases, of an altered common sensibility; what one fails to follow is the link between such alteration, and the new habits, of judgment, or of apperception, which tend to get formed upon this basis.

But I do not wish to burden you with a mere enumeration of problems, and I will not here further dwell upon the inadequacies of the current theories of the factors of self-consciousness, whether these theories lay stress upon the common sensibility, or upon the memory, as the principal factor in their explanations of the variations of the ego. It is only necessary to show that, while both the common sensibility and the memory are certainly largely concerned in the constitution of the self, the problem of self-consciousness is not thus to be fully solved. One must look to other factors as well. One has in fact only to remember that some large alterations of the common sensibility seem to involve very little change of self-consciousness at all, in order to see how complex the problem is.

And now, as to the real problem itself, it is surely one relating to the origin, to the nature and to the variations, of a certain important collection of mental habits. What are these habits? How do they arise? I insist, a mere catalogue of the contents of self-consciousness helps us little, unless we can interpret the facts in terms of the known laws of habit. For a man is self-conscious in so far as he has formed habits of regarding, remembering, estimating, and guiding himself. And now whenever these habits are in play, they all of them, as I must next insist, have a common and noteworthy character. If a man regards himself, as this individual Ego, he always sets over against his Ego something else, viz.: some particular object represented by a portion of his conscious states, and known to him as his then present and interesting non-Ego. This psychological non-Ego, represented in one's conscious states, is of course very seldom the universe, or anything in the least abstract. And, for the rest, it is a very varying non-Ego. And now, it is very significant that our mental habits are such that the Ego of which one is conscious varies with the particular non-Ego that one then and there consciously seems to encounter. If I am in a fight, my consciously presented non-Ego is my idea of my opponent. Consequently I am then conscious of myself as of somebody fighting him. If I am in love, my non-Ego is thought of as my beloved, and my Self, however much the chord of it pretends, trembling, to pass in music out of sight, is the Self of my passion. If I strut about in fancied dignity, my non-Ego is the world of people who, as I fondly hope, are admiring me. Accordingly I then exist, for myself, as the beheld of all beholders, the model. If I sink in despair and self-abasement, my non-Ego is the world of the conceived real or ideal people whose imagined contempt interests, but overwhelms me, and I exist for myself as the despised Ego, worthy of their ill will. When I speak, my non-Ego is the person or persons addressed, and my Ego is the speaker. If I suddenly note that, though I talk, nobody marks me, both the non-Ego and my Ego dramatically change together in my consciousness. These two contents of consciousness, then, are psychologically linked. Alone, I am so far not myself.

My consciousness of my Ego is a consciousness colored by my conceived relations to my endlessly changing consciousness of a non-Ego. And notice, I speak here as little of any metaphysically real non-Ego as I speak of any metaphysically real Ego. The whole question is here one of mental states and of the actual habits of their grouping not of relative, nor yet of real relations outside of consciousness. I point out merely the fact that, according as one chances to conceive thus or thus the non-Ego of his strongest current interest, even so, on the other hand, he conceives his Ego thus or thus, viz., as something related to this non-Ego, opposed to it, concerned in it, possessor of it, crushed by it, desirous of winning it, or however the play of habit and of interest makes the thing seem. Here, I think, lies the real key to all the variations of Self-consciousness, whether their conditions involve the common sensibility or not.

The psychological problem of self-consciousness reduces itself, then, to the following form. One must ask: How has one come to form all these habits of drawing a boundary, in one's consciousness, between mental states that represent a non-Ego, and mental states that clump themselves together into the central object called the Ego? One must also ask: Whence comes all this material for variation, whereby the content called the Ego shifts endlessly as the content called the non-Ego alters? And one must further inquire: How do the constitution and the variations of the Ego get that intimate relation to the sensations of the common sensibility upon which we have laid stress from the start?

Now to all these questions, as I hold, the recent study of childhood has tended to suggest at least a plausible answer. The substantial basis for the answer that I shall suggest has been reached, pretty independently, by my friend Professor Baldwin, of Princeton, and by myself. Professor Baldwin has given to some aspects of the matter, so far as concerns child life, a much fuller working out than I have done, both in his earlier papers and in his recently published book called *Mental Development in the Child and the Race*. On the other hand, in a recent discussion in the *Philosophical Review* (of Cornell) I have stated my own notions as to certain philo-

sophically important aspects of the growth of self-consciousness. But the application of these theoretical considerations to the study of the pathological variations of self-consciousness in the present paper is, I think, new.

The early intellectual life of the child is lost to us in obscurity, despite numerous recent observations. But we are clear that the infant, in the first months of life, has nothing that we should call self-consciousness. But the first clear evidence that we get of the presence of a form of self-consciousness intelligible to us comes when the infant begins to be observantly imitative of the acts, and later of the words, of the people about it. In other words, the first Ego of the child's intelligible consciousness appears to be, in its own mind, set over against a non-Ego that, to the child, is made up of the perceived fascinating, and to its feeling more or less significant, deeds of the persons in its environment. From this time on, up to seven or eight years of age, any normal child remains persistently, although perhaps very selectively, imitative, of deeds, of habits, of games, of customs, and often of highly ideal and perhaps quite imaginary models, such as are suggested to it by fairy-stories and other such material. As one follows the growth of these imitative tendencies, from their initial and quite literal stages, through those stages of elaborate impersonation and of playful, originally colored, often enormously insistent games, in which the child follows all sorts of real and fantastic models, one is struck by the fact that any normal child leads, relatively speaking, two lives, one naïve, intensely egoistic from our point of view, but relatively free from any marked self-consciousness in the child's own mind, while his other life is the life in which he develops his conscious ideas and views of himself as a person. The relatively naïve life is the life of his childish appetites and passions; the relatively self-conscious life is the life of his imitations and dramatic impersonations, of his poses and devices, of his games, and of his proudly fantastic skill, and of the countless social habits and attitudes that spring up from this source. The two lives mingle and cross in all sorts of ways. But the child who merely eats, cries, and enjoys his physical well-being, is not

just then self-conscious as is the child who plays horse, or hero, or doctor, or who carefully tries to follow a model as he draws, or to invent a trick as good as one that he has seen. The latter child, however, is essentially imitative, first of persons, then of ideas, then of the facts of the physical world as such. But the former child is simply the creature of natural impulses and passions, and would never come to self-consciousness, in our sense, if his life were not gradually moulded by the elaborate habits which the imitative child constantly introduces.

Now the psychological importance of imitation lies largely in the fact, that in so far as a child imitates, he gets ideas about the inner meaning or intent of the deeds that he imitates, and so gets acquainted with what he early finds to be the minds of other people. The child that repeats your words, slowly learns what they mean. The child that uses scissors, pencil, or other tools after you, learns, as he imitates, what cutting means, and what drawing, or other such doings. And as he thus learns, he gets presented to his own consciousness contents, which he regards as standing for those of your mind. The experienced interesting outcome of an imitated deed, is for the child the obvious meaning of that deed, for you, as you did it. But he does not get these contents,—these glimpses of your meaning,—he does not get them, at first, very easily. He gets them by persistently watching you, listening to you, playing with you, trying to be like you, all activities that for him involve muscular sensations, emotional concerns, and still other variations of his common sensibility. These efforts of his to grasp your meaning are marked and often delightful incidents of his consciousness. He returns over and over to his favorite games with you. He encounters every time your meaning, and he sets over against it those experiences of his own doings, whereby he comes to participate in your meaning. Here now the child always has present to him two sets of contents, both fascinating, each setting the other off sharply by contrast, while the contrast itself establishes the boundary between them. The first set of contents are his perceptions of your deeds, and his representation of your discovered mean-

ing in these deeds. The second set of contents are his own imitative acts themselves, as perceived by himself, these acts, and his delights in them. The first set of contents depend upon you. The child feels them to be uncontrollable. As perceptions, and as representations, these contents do not get closely linked to the child's common sensibility. They stand off as external although welcome intruders. On the other hand, the other set of contents, the child's own newly discovered powers, due to his imitation, are closely centred about his common sensibility, are accompanied with all the feelings which make up the sense of control, and get remembered, thenceforth, accordingly. The first set of contents form the psychological non-Ego of this particular phase of consciousness. The second set of contents form the psychological Ego corresponding thereto. One sees why the Ego-part of this sort of consciousness includes the common sensibility, and the sense of voluntary control, and why the non-Ego here involves contents that are set off by the contrast as uncontrollable, and as not closely linked to the common sensibility. And it is in this contrast that the source of true self-consciousness lies. We do not observe a given group of mental contents as such unless they are marked off by contrast from other contents. One could have all the common sensibility you please, and all the feelings of voluntary control, without ever coming to take note of this totality of united or centralized mental contents as such, and as clearly different from the rest of one's field of consciousness. Even now we all of us tend to lose clear self-consciousness so soon as we get absorbed in any activity, such as rowing, hill-climbing, singing, whistling, looking about us at natural scenery,—any activity I say, whose object does not, by the sharp contrast between its own external meaning and our efforts, call our attention to our specific relation to some non-Ego. Yet in lonely rowing and hill-climbing the common sensibility is as richly present as it is in many of our most watchfully self-conscious states. On the other hand, when I work hard to make my meaning clear to another man, or to make out what he means, I am self-conscious, just in so far as I contrast my idea of his ways and thoughts, with my own

effort to conform to his ways and thoughts. And just such an effort, just such a contrast, seems to mediate the earliest self-consciousness of the imitative child, and to secure the tendency of the self to be built up about the common sensibility, while the not self gets sundered therefrom. So then one sees the rule:—If one is keenly self-conscious, the common sensibility must be central. But, on the other hand, one may have a rich common sensibility without any keen self-consciousness. It is the contrast of Ego and non-Ego that is essential to self-consciouness.

But of course the child's relations to the varying non-Ego of consciousness do not remain merely imitative. When once he has other minds in his world, the function whose essence is the contrast between his conceptions of these minds and his view of his own response to them, can take as many forms as his natural instincts determine. His naïve life of appetites gets gradually infected by his conscious relations to other people. He wants good things, and perhaps must feign affection or show politeness, or invent some other social device, to get what he wants. Here again is an activity depending upon and bringing to light, the contrast between his own intention, and the conceived or perceived personal traits and whims to which he conforms his little skill. He learns to converse, and gets a new form of the contrast between the sayings of others (which he interprets by listening), and his own ideas and meanings. He reaches the questioning age, and now he systematically peers into the minds of others as into an endlessly wealthy non-Ego, in whose presence he is by contrast self-conscious as an inquirer. Here, every time one has the essential element of contrast upon which all self-consciousness depends. Argument and quarreling later involve similar contrasts. As to the external physical world,—what the child shall most care for in that, is largely determined for him by his social relations. Whatever habit he has acquired by social imitation, he can, therefore, in the end, apply to things as well as to persons. As a fact he is notoriously often animistic, directly transferring social habits to physical relations, and regarding things as alive. And here again he becomes self-conscious, by contrasting his own

activities with the conceived natures and meanings of external things. I do not at all suppose that the child regards all natural things in an animistic way; but I am of opinion, for reasons which I have set forth elsewhere, that our whole tendency to distinguish as sharply, as we all now do, between the self and the external physical world, is a secondary tendency, due in the child's case, to social influences. It is language, it is the accounts that people give to us of things, it is the socially acquired questioning habit,—it is such things that extend the contrast between Ego and non-Ego, at first mainly a social contrast, to the relations between one's own mind and one's physical environment. Even now, as I just pointed out, if we forget that nature is full of thinkable mysteries, of meanings, of laws, of other ideal contents whose significance we do not comprehend,—if we forget this, and lapse into mere busy and absorbing physical experience, as when climbing hills alone, or rowing, or cheerily whistling as we walk, we forget to be self-conscious, just because we lose sight of the sharper contrasts of Ego and non-Ego.

III.

But, to return to the explicitly social relations, there is still another factor to note in our early relations to our conceived social non-Ego. And this is the fact that, by our instinctive mental constitution as moulded by our social habits, we are early subject to a vast number of more or less secondary emotions, each one of which involves large alterations of the common sensibility, while all of these particular emotions arise under circumstances which make explicit the contrast between one's self, and one's idea of one's fellow's mind. Such emotions we get as children when people praise us, blame us, caress us, call us pet names, stare at us, call us by name, ask us questions, and otherwise appeal to us in noteworthy ways. Such emotions too we get again, in novel forms, in youth, when the subtle coloring of the emotions of sex begins to pervade our whole social life. Such emotions are shame, love, anger, pride, delight in our own bodily seeming as displayed before others, thrills of social expectation, fears of appearing ill in the eyes of others.

Such emotions involve blushing, weeping, laughter, inner glow, visceral sensations of the most various kinds, and feelings of the instinctive muscular tensions related to our countless expressive social deeds. These experiences are, however, aroused by situations all of which essentially involve the aforesaid contrast between our own ideas, wishes, or meanings, and the conceived states of other minds. Hence these emotional states associate themselves, as variations of the common-sensibility, first, with social situations, *i. e.*, with cases where Ego and non-Ego are sharply contrasted; and then especially with the Ego-member of the relation of contrast. And so, altogether by the force of habit, these emotions, which if primarily aroused would be mere content, belonging neither to Ego nor to non-Ego, come to be the specific emotions of self-consciousness, so that now whenever we have just these emotions, from any cause whatever, we are at once keenly self-conscious,—and that merely because the emotions in question faintly or keenly suggest particular social situations. Emotions that have had no such constant relation to social situations, involve no such marked states of self-consciousness. Fear of physical dangers tends to diminish our self-consciousness; shame intensifies it. Yet keen physical fear, as the more primitive emotion, involves vaster commotions of the common sensibility than does shame. Were then the marked presence or variation of the common sensibility in consciousness the sole and sufficient cause of the presence or of the variation of one's immediate or sensory Ego, physical terror would make one more self-conscious than does shame. But panic fear, in its intensest conscious forms, involves rather a destruction than a positive alteration of self-consciousness; while the most abject shame grows the more intensely self-conscious as it gets the more marked. Why? Because shame, habitually associated only with social situations, suggests them even where it is pathological and is not due to them; and so it brings to consciousness the contrast of Ego and non-Ego.

Thus, then, it is that I propose to explain what the current theories of self-consciousness usually seem unable to deal with, viz., the before-mentioned fact that certain pathological

variations of the common sensibility profoundly alter the tone or constitution of a patient's self-consciousness, while others, equally intimate and vast, either leave self-consciousness relatively intact, or simply put it wholly out of sight without first tampering with its integrity. When a man has the colic he does not say, 'My Ego is deranged.' His account of the case is far less metaphysical. But when, as in the depression after the grip, he has certain very much dimmer and more subtle alterations of the common sensibility, he may complain of precisely such a sense of alienation from himself. Why? Well, as I should say, the colic suggests no social situation; the vague depression after the grip may dimly suggest, by habit, situations of social failure, or confusion, or powerlessness, such as, from sensitive childhood until now, have played their part in one's life. The suggestion may be very faint, and utterly abstract. No particular failure, no special case of social helplessness, comes to mind. But our nascent associations can be present in all degrees of faintness; and here I maintain are associations dimly involving social contrasts between Ego and non-Ego. Here, then, are conditions for the function of self-consciousness.

Since the emotional alteration of the common sensibility has thus the most various habitual relations, now with our unsocial physical states as such, now with social activities, one sees how it is possible for a nervous sufferer to say, on one day, that he personally feels his very being wrecked, and his self-hood lost or degraded, while on another day he may simply declare that he suffers keenly, but regards the affair as a mere physical infliction, external to his central self-hood. In the physical sufferings of sensitive women this shifting of the enemy's ground from the region of the physical or psychical pain felt as a mere brute fact, hateful but still bearable, to the region where the sufferer complains of an intolerable loss of self-possession, is notoriously a common and, to the sufferer herself, a puzzling incident. Both times the common sensibility is deeply affected, often in ways not subjectively localizable; the difference, I think, must be due to the nascent associations of the common sensibility now

with ideas of social situations, now with ideas of unsocial bodily events. There are some chronic neurasthenic sufferers who, despite headaches, spinal pains, and other distorted sensations innumerable, preserve for years a marvelous self-possession in face of their disorder; very many other such nervous sufferers, of the same general type, are throughout self-consciously cowardly and abject. One cannot assert that the latter class are more deranged in common sensibility than are the former. But many a neurasthenic man has really little to complain of *except* the unspeakable wretchedness of his deranged self-consciousness. How can one explain such phenomena without resort to the principles of habit and association? The social habits, however, of the type now defined, at once furnish a *vera causa* for the interpretation of some sensory disturbances as alterations of self-consciousness, while other disturbances, equally great and vague, get interpreted by the sufferer as merely external events. To be sure we cannot yet give an exhaustive classification of the variations of the common sensibility into those closely associated with social situations, and those not associated, or but slightly associated, yet the contrast of physical fear and of shame has already shown us that such a classification might, with care, be more or less worked out. We know, for instance, that the sexually tinged emotions normally have very complex social associations. Consequently, we may expect to find self-consciousness especially deranged in disorders involving the sexual functions. This expectation seems to be abundantly verified, even in ordinary cases of disorder, such as the teacher of youth may sometimes see as well as the doctor; and if one wants more verification, one may get it at will from the monumental records that fill Krafft–Ebing's too well-known and ghastly book. On the other hand, a sufferer from the emotional states accompanying ordinary physical exhaustion, or from some forms even of grief, or from a severe cold that does not give the form of depression now associated with the grip, or from some forms of even violent headache, often wonders how much pain and emotional alteration he can endure without any proportionate alteration of self-consciousness. And

these states are precisely such forms of consciousness as are not so closely associated with social situations. Finally, the emotions connected with laughter furnish an almost perfect natural experiment for our purpose. There are three principal sorts of laughter: the laughter of mere physical gleefulness, such as appears much in children, less in adults; the laughter of scorn, and the laughter of the sense of humor. The first is not an especially self-conscious affair; but the laughter of scorn and of a sense of humor are both of them always keenly self-conscious, involving what Hobbes called 'sudden glory in him that laugheth.' The emotions of the two latter types involve social situations, present or suggested. I shall find no time to point out at any length the application of the foregoing analysis to the study of the associative alterations of the socially tinged self-consciousness in true melancholia, in mania, or in the exaltation of general paralysis. But the mention of such alteration of the self brings us at once to the next and final stage of our inquiry.

IV.

I have so far spoken of self-consciousness as it appears in more or less explicitly social relations. But, one may reply, "Are we not, at pleasure, self-conscious when we are quite alone? Does not one reflect, does not one judge one's self? Is lonely meditation free from self-consciousness? Is not conscience a self-conscious affair? And yet in such cases does one contrast an Ego with any literal non-Ego? In such processes is not the Ego explicitly related to just the Ego, alone by itself? And are there not, in the phenomena of insanity, many alterations of this sort of purely internal self-consciousness?" I reply at once that my theory is precisely *that habits once acquired in social intercourse can and do hold over when we are alone, and can then apply within the content of one's own mind.* The transition is simple. First I can dramatically remember my actually past imitative deeds, my quarrels, my successful social feats, my chagrins, my questionings, my criticisms of others, and the bearings of others towards me. In all such cases I am self-conscious over again in memory, by virtue of our now familiar contrast-

effect. Further, as just seen, my emotions can vaguely suggest social situations, indefinite in character to any degree. By coalescence, a vast group of social habits of judging others, and of feeling myself judged by them, can get woven into a complex product such as is now my conscience. Conscience is a well-knit system of socially acquired habits of estimating acts—a system so constituted as to be easily aroused into conscious presence by the coming of the idea of any hesitantly conceived act. If conscience is aroused in the presence of such a hesitant desire to act, one has, purely as a matter of social habit, a disposition to have present both the tendency to the action, and the disposition to judge it, standing to one another in the now familiar relation of Ego and non-Ego. Which one of them appears as the Ego, which the non-Ego, depends upon which most gets possession, in the field of consciousness, of the common sensibility. If the tendency to the estimated act is a passionate tendency, a vigorous temptation, and if the conscientious judgment is a coldly intellectual affair, then the situation dimly reminds me of cases where other people, authoritative and dignified rather than pleasing, have reproved my wishes. Conscience is then the colder non-Ego, the voice of humanity, or of God. My common sensibility merges with my passion. The reproof perhaps shames me; yet *I* want to have my way; only that other, that authoritative inner non-Ego, my conscience, will not let me go free. But if, on the other hand, the conceived act is less keenly desired, and if my conscientious plans are just now either fervently enthusiastic or sternly resolute in my mind, then it is my conscience which merges with my common sensibility, and I myself am now, in presence of the conceived act, as if judging another. I feel then secure in my righteousness, and I look with disdain upon that which would tempt me if I were weaker, but which now is a mere non-Ego. It is in a similar fashion, by a dramatic imitation not of actual, but of abstractly possible social relations, that I can question myself, and wait for an answer, can reflect upon my own meaning, can admire myself, love myself, hate myself, laugh at myself, in short do or suffer in presence of my own states and processes whatever social life

has taught me to do or to suffer in presence of the states and processes of others. In every such case the central Ego is so much of my conscious process as tends more to merge with the common sensibility. My inner, but more peripheral, relative non-Ego is so much of my conscious process as tends more to resemble, in interest, in general tone or in uncontrollable unexpectedness, the experiences which, in ordinary social life, are due to other people. Yet since all these inner contrasts are constantly corrected by my habits of external perception and of memory, which remind me all the while of a literal non-Ego outside of all these processes, this inner sundering normally remains only, as Professor Ladd has called it, dramatic—a sort of metaphor, which I can correct at pleasure, saying at any moment, "but all this is merely Ego, after all. The real non-Ego is the world of live other people yonder."

Thus the normal inner life of reflection, of conscience, of meditation, and of the so-called 'spiritual Ego' in general, is simply, in us human beings, an imitation, a brief abstract and epitome, of our literal social life. We have no habits of self-consciousness which are not derived from social habits, counterparts thereof. Where the analogy of our relations to our fellows ceases, reflection ceases also. And this is precisely what constitutes the limitation of our reflective processes in philosophy and in psychology.

But surely, if this summarizes the conditions of our normal self-consciousness, when we are thinking alone, it also gives room for indefinitely numerous abnormal variations. Suppose that there appear in the conscious field hallucinations of the muscular sense, of the sort so well described in Cramer's noted monograph. Let these be motor speech hallucinations. Then the patient may observe the puzzling phenomenon that, whenever he thinks, there is some mysterious tendency present that aims to objectify his thoughts, in spoken words. Somebody or something either takes his own thoughts away from him and speaks them, or forces him, willy nilly, to speak them himself. The thoughts are his own. The sounding of them forth, in this way, is not his. His thoughts run off his tongue, get spoken in his

stomach, creak out in his shoes as he walks, are mockingly echoed or in the end commented upon by another power. This other power, this stealing of his thoughts, involves of course a deep disturbance of his self-consciousness, which tends gradually to pass over into a regular system of delusions. Yet what does the process mean? It means, at first, merely the appearance of uncontrollable elements of consciousness, which by virtue of the habits connected with the uncontrollable in general cannot get merged in the common sensibility, and which are yet in a problematic and painfully intimate relation to what he does recognize as his own. This foreign power need not for a good while behave *enough* like the true voice of another to become a genuine hallucinatory comrade or enemy, as it would do and does if the patient hears his voices without of himself recognizing their close relation to his stream of thought. But in this uncontrollable hallucinatory thinking aloud there is enough suggestion of the foreign to make the patient feel that his own thoughts are getting somehow estranged from him. That these are his own thoughts he at first knows, by virtue of the general contrasts between real Ego and real non-Ego still present to him. That they are getting estranged he knows, for that is to any one a relative non-Ego which behaves more or less as one's original social non-Ego, one's fellow in society, behaves. His behaviour is relatively uncontrollable; and so is here that of the patient's thoughts.

Or again, suppose that one's depressed emotional condition, as in melancholia, or at the outset of a delirium of suspicion or of persecution, contains emotions resembling the normal emotions of conscientious guilt, or the feeling of social dread. Then these feelings tend to assimilate in one's actual surroundings, or in one's memories, data which suggest, to one patient an actually believed social condemnation of his deeds, or an actual judgment of his inner conscience passed upon his sinfulness, while to another patient his own sorts of emotion suggest an especially hostile scrutiny of his appearance by the passers by, or an inner sense that he must hide from possible scrutiny. On the other hand, feelings quite the reverse of these suggest to the exalted general

paralytic whatever remembered or fancied social relations, expressing his vast powers, the fragments of left-over social habits which still survive in his chaos permit him, in passing, to express.

Or, once more, another patient has present to consciousness two or more streams of feelings, impulses, thoughts, which are sharply contrasted with one another, while the portions of each stream more or less hang together, by virtue of common contents or tone. All of these streams belong to his general Ego,—this he recognizes by the normal contrast with the actual external world. But meanwhile they have their inner contrast, which is no longer, like the just mentioned contrasts in normal consciousness, a source of merely dramatic metaphor. This abnormal contrast is intense, uncontrollable, continuous. Now let the reflections or the context of these streams be such as in any fashion to remind the patient of any social relation, contest, rivalry, quarrel, criticism, pity, questioning, discussion; and then the patient can only say: 'There are in me two or more selves, I am divided.' If one of the streams involves more of the common sensibility than does the others, or more of the sense of control, the patient may speak of the less favored streams as other selves, or as the 'Other Fellow' without having any full-fledged delusion of a real outside oppressor. And in all this there will be mere associations of ideas, mere socially acquired habits,—no new mysteries of self-hood whatever. Yet how complex the physical and psychological background of such abnormal habits may be, I will try to illustrate, as I close, by a single case.

(*To be concluded.*)

ON DREAMING OF THE DEAD.

BY HAVELOCK ELLIS.

London.

We know that among savages in many parts of the world it is held that the soul in dreams leaves the body, to wander over the earth. Tylor and others have further shown that dreams play an important part in the evolution of belief in an after-world. So far as I am aware, however, no one has ever asserted that there is a peculiarity in the mechanism of dreams of the dead which powerfully suggests that our dead friends have only apparently died. For some years I have given attention to the psychology of dreaming, and collected the observations of my friends, and I wish here to draw attention to a certain type of dream, and to point out its significance from a primitive point of view, as a factor in the wide-spread belief that death is only a transitory and apparent phenomenon. I am only able to present two series of dreams, occurring to two individuals, intimate friends of my own. The individuals in question are very unlike each other in character and temperament; the dreams were carefully recorded at the time (an indispensable point in the study of dreaming); and it is possible to exclude entirely the influence of suggestion, as each dreamer's dreams were unknown to the other dreamer.

Observation I.—Mr. C., age about 28, a man of scientific training and aptitudes. Shortly after his mother's death he repeatedly dreamed that she had come to life again. She had been buried, but it was somehow found out that she was not really dead. Mr. C. describes the painful intellectual struggles that went on in these dreams, the arguments in favor of death from the impossibility of prolonged life in the grave, and how these doubts were finally swallowed up in a sense of wonder and joy because his mother was actually there, alive, in his dream.

These dreams became less frequent as time went on, but some years later occurred an isolated dream which clearly shows a further

stage in the same process. Mr. C. dreamt that his father had just returned home, and that he (the dreamer) was puzzled to make out where his mother was. After puzzling a long time he asked his sister, but at the very moment he asked it flashed upon him—more, he thinks, with a feeling of relief at the solution of a painful difficulty than with grief—that his mother was dead.

Observation II.—Mrs. F., age about 30, highly intelligent but of somewhat emotional temperament. A week after the death of a life-long friend to whom she was greatly attached, Mrs. F. dreamed for the first time of her friend, finding that she was alive, and then in the course of the dream discovering that she had been buried alive.

A second dream occurred on the following night. Mrs. F. imagined that she went to see her friend, whom she found in bed, and to whom she told the strange things that she had heard (*i. e.*, that the friend was dead). Her friend then gave Mrs. F. a few things as souvenirs. But on leaving the room Mrs. F. was told that her friend was really dead, and had spoken to her after death.

In a fourth dream, at a subsequent date, Mrs. F. imagined that her friend came to her, saying that she had returned to earth for a few minutes to give her messages and to assure her that she was happy in another world and in the enjoyment of the fullest life.

A third dream occurred more than a year later. Some one brought to Mrs. F., in her dream, the news that her friend was still alive; she was taken to her and found her as in life. The friend said she had been away, but did not explain where or why she had been supposed dead. Mrs. F. asked no questions and felt no curiosity, being absorbed in the joy of finding her friend still alive, and they proceeded to talk over the things that had happened since they last met. It was a very vivid, natural and detailed dream, and on awaking Mrs. F. felt somewhat exhausted. Although not superstitious, the dream gave her a feeling of consolation.

I have made few inquiries as to the frequency of this type of dreams. It does not appear to occur to every one. I can, however, record a slight personal observation.

Observation III.—I dreamed that I saw a dead friend, the editor of a psychological journal, alive and well in his room, together with two foreign psychologists also known to me, who had apparently succeeded him in the editorship of the journal, for I saw their names on the title-page of a number of it which was put in my hands. It surprised me that, though alive and well, he should have ceased to edit the journal; the theory by which I satisfactorily accounted to

myself for his appearance was that, though he had been so near death that his life was despaired of, he had not actually died; his death had been prematurely reported. It flashed across my dream-consciousness, indeed, that I had read obituaries of my friend in the papers, but this reminiscence merely suggested the reflection that some one had been guilty of a grave indiscretion.

This personal observation is inconclusive, the central difficulty of the situation being too easily eluded; the dream, however, clearly belongs to the same type and illustrates the flimsy nature of the explanations which satisfy us in dreams, provided they accord with the image actually present to consciousness. It would be interesting to know if this type of dream is as common as my observations suggest, and also to ascertain the nature of the dream-process regarding the dead among remote and uncivilized peoples.[1]

It does not seem difficult to account for this dream-process and for its frequency. This dream-type is only a special variety of the commonest species of dream, in which two or more recent but totally unrelated reminiscences flow together and form a single bizarre congruity, a *confusion* in the strict sense of the word. The death of a friend sets up a barrier which cuts into two the stream of impressions concerning that friend. Thus two streams of images flow into sleeping consciousness, one representing the friend as alive, the other as dead. The first stream comes from older and richer sources; the second is more poignant, but also more recent and more easily exhausted. The two streams clash

[1] In Japan stories of the returning of the dead are very common. Hearn gives one as told by a Japanese which closely resembles the type of dream I am discussing: "A lover resolves to commit suicide on the grave of his sweetheart. He found her tomb and knelt before it and prayed and wept, and whispered to her that which he was about to do. And suddenly he heard her voice cry to him 'Anata!' and felt her hand upon his hand; and he turned and saw her kneeling beside him, smiling and beautiful as he remembered her, only a little pale. Then his heart leaped so that he could not speak for the wonder and the doubt and the joy of that moment. But she said: 'Do not doubt; it is really I. I am not dead. It was all a mistake. I was buried because my parents thought me dead—buried too soon. Yet you see I am not dead, not a ghost. It is I; do not doubt it!'" It is perhaps worth mentioning that the incident told in the Fourth Gospel (Ch. xx., vv. 11–18) as occurring to Mary Magdalene when at the tomb of Jesus, recalls the dream-process of fusion of images. She turns and sees, as she thinks, the gardener, but in the course of conversation it flashes on her that he is Jesus, risen from the tomb.

in dream-consciousness, both, from the inevitable conditions of dream-life, being accepted as true, and they eventually mix to form an absurd harmony, in which the older and stronger images (in accordance with that recognized tendency for old psychic impressions generally to be most stable) predominate over those that are more recent. Thus my friend in Observation I. seems to have begun his dream by imagining that his mother was alive as of old; then his more recent experiences interfered with the assertion of her death. This resulted in a struggle between the old-established images representing her as alive and the later ones representing her as dead. The idea that she had come to life again was evidently a theory that had arisen in his brain to harmonize these two opposing currents. The theory was not accepted easily; all sorts of scientific objections arose to oppose it, but there could be no doubt, for his mother was there. The dreamer is in the same position as a paranoiac who constantly seems to hear threatening voices; henceforth he is absorbed in inventing a theory (electricity, hypnotism, or whatever it may be,) to account for his hallucinations, and his whole view of life is modified accordingly. The dreamer, in the cases I am here concerned with, sees an image of the dead person as alive, and is therefore compelled to invent a theory to account for this image; the theories that most easily suggest themselves are either that the dead person has never really died, or else that he has come back from the dead.

I think it worth while to record these phenomena, leaving others to prove or disprove their frequency. Such dreams seem to make a deep impression on the dreamer, even in an age in which no supernatural significance is attached to dreams.[1] If, as I venture to suggest, such dreams have an organic foundation, which causes them to occur with some degree of frequency, they must in more primitive times have constituted a significant factor in the evolution of culture.

[1] I may, however, refer to the fact that in insanity dreams are still occasionally an important influence. For references to hallucinations begun in sleep and finally accepted, see Féré, 'The Pathology of Dreams,' *Brain*, vol. IX (1887), p. 488. A dream, it has lately been shown, played a prominent part in the development of Cowper's insanity.

EMOTION, DESIRE AND INTEREST: DESCRIPTIVE.

BY S. F. M'LENNAN.

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After all that has been written in regard to emotion it may seem superfluous for any one to take up the subject again. But inasmuch as one can still hear a rumbling in the distance and the different parties to the strife continue it in what one might call an exegesis of what they did mean in certain of their writings, it may be permitted to bring forward a few thoughts suggested partly by the controversy. The general descriptive outline to be attempted may not contain many things new —perhaps, as isolated statements, none. But it seems to the writer that these points may easily bear a good deal more looking into and may be carried into adjacent fields with the result that new light will be thrown upon the subject. Though cheerfully acknowledging the influence of all that has of late been written upon the matter in hand, it is but fair to state that the general conclusion in regard to emotion, although the same as that maintained by Professor Dewey, yet was worked out independently by the writer and in outline was formulated at the very beginning of the late strife. Of this, Professor Baldwin, to whom the outline was first communicated, can bear testimony. But leaving this aside, I hope that what is to be said may vindicate its own appearance as regards emotion and may throw some light upon the other topics.

The problem in hand is a description of the nature and inter-relation of emotion, desire and interest.

When one begins to ask himself what concrete emotional experiences mean a difficulty at once arises. Are we dealing with emotion, desire, or with a fact of interest? At first sight our way seems clear, but a closer inspection shows that our statement must shift about. An element of desire may be contained right in the centre of the concrete whole denomi-

nated emotion, *e. g.*, hate. At the same time it may equally well be termed, what it really is, a matter of interest. What, then, are we to make of it? Of course the old pigeon-hole theory has been put to rest, and we know that all mental states are interconnected and should expect that no one would be shut off from the others. But this free and easy way of settling the question will scarcely do, and we shall simply be passing over something which will help us clear up our ideas. Suppose, for instance, that we enquire into two facts, ever old, ever new, facts that have caused much laughter and much pain, but which, after all, show themselves to be fundamentally interesting. I refer to the well known examples of love and hatred. These have figured often enough in description and will serve for the present as well as any others. By this time the human race should be fairly familiar with these facts, and yet when we try to make clear to ourselves what they really mean we find it no easy matter. Is either an emotion, a desire, or is it interest?

They seem to be all three. All know how 'interesting' for those concerned these different feelings are. They lie at the very roots of the life and stir up the whole being. However, instead of being regarded primarily as facts of interest they are customarily referred to the realm of emotion. But the moment we look more closely this will not do. They are just as truly desires as any facts could well be. To make this clearer let us try and analyze the experiences somewhat.

Take love first. Before the fountains are opened up there must be some primary interest. Those who do not attract our attention in some way call out no feeling. But again, many who interest us in this primary manner open up no fountains of affection. Let the interest, as is often said, become deeper, and before the parties know they are in Cupid's toils; there is a general upset, and often they seem to an outsider to be beside themselves. The general turmoil of feelings has been so often dwelt upon in poetry and in prose that I need do no more than simply mention it. We see plenty Romeos and Juliets round us yet who confirm the statement, and if any are doubtful a little consideration of present or past experience will give them some light. This stage of the ex-

perience, while most interesting, is truly described as emotional. But this is not all. In his love the youth is not satisfied with being worked up, with having his brain in a whirl, with hearing the clap-clap of his unruly heart. As part of his love he desires to possess his idol. The absence of the fair one is misery to him; his whole self demands that he should obtain and love her for his own. Love would not be the tremendous engine that it is were it not for this. Even when the emotional element predominates the element of desire is seen really to enter into the experience. But as the inner conflict becomes harmonized and settles down into a definite outgo, the account shifts, the emotion as such ceases, and desire becomes the prominent thing.

We now pass to the third stage. When desire is realized, and the youth has attained his end, does the love then cease? When such cases come before us we feel at once that the genuine experience was not there; we feel that in the full experience that the possession but continues and strengthens the regard. Love, then, truly becomes itself, and, amid the storms and stress of life, shows that nature which fills us with wonder and reverence when we come in contact with a fair example of it. How deep and lasting is the love of a parent for a child, of a husband for the mate who has stood by him, and with him weathered the many storms in life's journey. Yet in neither of these cases do we find the strong upheaval of early days. Nor again is the present experience one of desire, for the object has long been obtained. It is neither of these forms, but it is still love, love which has 'set' itself in the very citadel of life. We are brought back to interest once more, but this time not a passing interest, rather the deepest thing in life. Interest here is seen to be at the beginning, to underlie, and to be at the end of both emotion and desire. Out of it they arise and to it they return.

We shall next turn to the negative view. Here we shall notice the same inter-relation. Hatred arises out of a negative interest. It takes some little nagging, some little fooling about, some thwarting of purpose and treading on toes before 'our feelings are aroused,' as we say. When our temper is up and we hate the person, as we look at the matter from the

one side there is the same upsetting of ourselves. Our feelings storm and toss; they seem to overpower us, as swell after swell rushes on. But here, just as truly as in the case of love, we find a desire to harm or repulse the one who has injured us, bedded in the very nature of hate. As our inner life becomes steadied, this comes quite prominently into view. The account moves over to the desire side, and the state becomes one almost entirely of wishing to get even with the offender. This truly is bad enough, but human nature has deeper depths still. The momentary desire to harm may settle down into a set line of opposition ingrained in our nature. Hatred has fully become itself, shown fully its inner nature. The rush and swirl are past and gone; there is no desire of getting even in any particular way, but a steady persistent outgo of opposition which knows no change. Such a hatred never lets up, and follows to the very grave. Here, too, we return to interest—a negative interest.

After this general description a closer analysis of these states will be of value. The relation of emotion and desire may first come up.

As we can easily see, both are organic wholes in which several aspects may be detected. Again they are dynamic things. No very great acquaintance with the nature of either is necessary to see this. The organic unity of emotions, especially as containing several aspects, has often been overlooked. In psychologies the feeling element has been made prominent. One is led to believe that it is the whole thing, and besides is simply an accompaniment of the thought process. Out of this it seems to me a great deal of the present confusion has arisen. Now let us take the concrete facts, and staying closely by them, see what they mean. Take which emotion or desire you will, and it is evident at once that it is not made up of parts set off from one another. These parts are aspects of one living whole. Further emotions and desires belong to the reactive consciousness. As distinguished from volition, they represent an involuntary reaction of our nature; as distinguished from impulse—a certain solidity or definiteness of outgo. Within this agreement as to unity, dynamic force and reflex nature we shall later on

call attention to an essential distinction between the two states. Here we note that our nature rises up without being bidden, and, more than this, it usually directs whatever bidding there may be. Whenever the stimulus is present these phenomena immediately show themselves as driving, pressing, impelling and moving. Often we strive voluntarily to hold them in check, and find trouble.

Turning to an analysis of the different aspects by each state, emotion comes first to hand.

In the nature of emotion there is inner strife and yet unity. Our nature as a whole arises in answer to some stimulus, but the answer is a conflicting one. There is a lack of equilibrium, our nature sways to and fro, seems rent asunder, but all the time seeks to come to harmony. Instinctive tendencies pull together and apart. The character of the emotion is determined by the general nature of the strife. Anger is most keenly felt when our tendency to thrash the other person is held in check by the suggestion that we are not quite equal to the task, and had better not start in. When we are badly frightened the tendency to run, and even the running itself, is inhibited by weak knee reaction, due to the thought of danger. Every time the thought of this comes with a pulse upon us we seem stopped up. Emotion would thus show itself to be an instinctive 'preparing' for action—in which there is lack of harmony or coördination. As regards the moments of emotion, we have (1) a content. That every emotion has an intellectual element is quite easily seen by examining any of the concrete states known by this name. We see it in hate, anger, joy, dread, and all the rest, readily distinguishing them from one another. We may say, indeed, that the emotion terminates upon some external aspect, and may think that it is simply an accompaniment. This may be all very true, but it is just as true that in the emotion, as part of it, there is a content or object—this very situation as it is for us as interpreted. In hatred as an experience there is the idea of ourselves injured, and of the offender as reckoned with. Take all the emotions in succession and the same thing is found.

The question here becomes interesting as to how this content arises. Is the object (our interpretation) there immediately or is it built up? Consciousness soon makes this clear. The object is built up dynamically by our reaction; we receive a stimulation, a suggestion; and our progressive interpretation, our grasping of the meaning makes the situation what it is to us, as expressed in intellectual terms. Not until the interpretation is complete, till we see what the thing means is the emotion what it is for us. The emotion, as a whole, and in its aspect of content varies just as the interpretation. An act may arouse very angry feelings at the time, but seen in another light may cause joy. The joy and anger are what they are, as our way of looking at the matter is, as we perceive that the situation will or will not fit in with our life and is for our weal or woe.

(2) An Attitude. The content represented our intellectual valuing of the stimulus. Every stage of this valuing has another side which plays its part, and at every turn hands in its result for the construction of the situation for us. As we look closely at any emotional state we see that immediately, instinctively, we take up an attitude toward or against the stimulus as we make it out to be. At the suggestion of harm we are up in arms at once. When some one tramps on our toes we feel like hitting him, *i. e.*, we have an attitude toward him. The moment, however, that we notice that it was accidental, and apology is made, our attitude sweeps round and we say that it is all right. This shows that the attitude, too, is a relative thing, varying according to the suggestion. As we study our own states it is quite wonderful to notice how our attitudes sweep about. A word, a little incident, may be sufficient to give the whole experience a different coloring. As to the character of the attitude, it is determined by the nature of the person. This shows at once. Take for example the case of grief. One person is literally crushed, and cannot stand the strain, another may explode in angry denunciation; a third, while 'cut to the quick,' as we say, takes up his burden and plods along his weary way. In fact, here all manner of natures are shown. There are those whose emotions seem like

the foam on the sea: they are all a-bubbling and a-gushing, but there is no stability, and we turn away weary. We feel that there is weakness, a lack of determination, strength, force of character. Other natures may be slow to arouse, but are firm and steady. When the emotions of such are aroused we are conscious that there is some meaning to them. If they are opposed to us we at once begin to gird ourselves for the fray. We respect if we do not love. This strength and solidity of the emotion often goes together with a keenness and fineness of reaction—complexity and stability build themselves together. Now this instinctive reaction, of whatever character it may be, dynamically and progressively builds up as the interpretation of the situation goes on. It, too, reacts upon the suggestion and modifies it. The valuation becomes what it is as much from our attitude as from anything else. It becomes what we instinctively feel we can make of it. To one person the situation is one to be avoided; to another to be entered into as he finds he cannot or can make something of it.

Here, also, we find two great lines of cleavage—one an innate tendency to absorb the new situation, to make it part of our life; the other to avoid it, throw it off, or keep it from us altogether. These two attitudes show themselves as attraction and repulsion. In our love we naturally 'go' to persons; in our hatred we seek to keep them away or to get rid of them altogether. Whatever seems good to us and fits in with our own life we go toward; from whatever seems evil or will harm us we turn away.

(3) Beside the intellectual and attitude aspects there is something else to be considered, viz., the swell or drive of feeling. This we are all clearly conscious of. It seems as if our whole nature were boiling up, or as if a cold, frigid hand had laid its icy grip upon us. This swell or drive of feeling influences us in two distinct lines, as the others do, and contributes its share in a very material way to the experience in whole and in part. It seems a direct and immediate organic answer to the stimulus as it is being interpreted. When we think that some injury has been done us, and we resent it, our very blood appears to boil up within us, and

the more we think of it the worse it becomes. Every new feature stirs us up more, and our feelings are like oil added to the fire. Our whole nature flames up and becomes colored with the burning. Our attitude becomes more definite and the intellectual valuation more clearly set forth. As wave after wave of tumultuous feeling comes rolling in upon us our hands clench harder and our injury seems greater. The emotion as a whole is filled in.

This carries us to (4) the color tone. We have noted that emotion was a state in which we were in unstable equilibrium, various tendencies were at war with one another, and gave the qualitative determination to the state. In all the three aspects which we found within this dynamic whole two great lines of cleavage show up. In the intellectual side there was the suggestion of weal or woe; as to attitude, an impulse toward or away from, for or against; on the side of feeling, elevation or depression, expansion or contraction of life. When we turn to the pleasure and pain coloring the same appears. Pleasure attaches to those states in which we find an idea of good, an attitude toward, a feeling of expansion, while pain attaches to the opposite. These various states fluctuate a great deal, and emotions may rapidly alternate or mix up. But if we watch closely we can easily see that as the 'cue' of the emotion is so is the tone.

Enough has been said to place before us the nature of emotion. The next thing to call attention to is its transformation. When emotion has been aroused in any individual we always notice that attention is called out. We endeavor to harmonize the conflicting elements, so that in unrestrained action they may pour forth. A deliberative state of affairs is at once brought on. When this is ended, and harmony has come, when we know what we are going to do, the state passes over into volition. Now, if the action is one which we may immediately carry out, the subjective determination is made objective by gripping on, by our fulfilling the conditions upon which our past experience has shown that the expression depends, or in searching for new combinations whereby expression may be brought about. But if we cannot immediately put our determination into effect the volition becomes

a harmonized way in which we are prepared to react on stimulation. As such it passes over into desire. Emotions then tend to pass into harmonized immediate action or volition, and into desire as instinctive 'preparedness' for action. We must note, however, that this preparedness does not 'set' immediately, and any new suggestion may bring about the old turmoil. Perhaps, after the strain of some severe conflict, in which our inner life seems torn to pieces, we arrive at some conclusion. We seem to be settled down, and suppose that our nature will at once answer in a steady outgo. How often we are deceived. A new point of view will start up the whole turmoil again. Until desire is 'set' it may pass back into emotion, and we find a continual vibration between the two.

It is now time for us to pass from the consideration of emotion to that of desire. Many things which have been said above have already given the outlines of what must now be set forth in fuller form.

Desire, like emotion, is a dynamic whole. Our nature in strong, definite lines goes surging and charging forth. Here, too, we have a good example of reflex activity. Upon the presence of a stimulus there is an immediate outgo of ourselves—a pressing, driving outward, often in such tumultuous fashion, indeed, that it seems impossible to hold the reins over the steeds in their wild career. In emotion we found inhibition. Here, too, it is found, but not in the same place. In emotion there was inhibition within the state itself. There was simply a preparing for action. In desire there is no lack of harmony within the experience. Our nature pours forth in harmonized, though often tumultuous swell. The inhibition is to the reaction seeking to express itself. We instinctively know what we want to do—our nature pours out to this, but there is some stoppage, some hindrance, and we feel pent up, our reaction cannot discharge itself. The stronger the inhibition the stronger our desire waxes, swelling and pressing forward until a limit of impossibility is reached and the whole is violently crushed. To take a simple case. When we are far from home, and the thought of those there is borne in with force upon us, our nature reacts and we

go forth to them. But something stops us. At such moments our longing grows stronger, and we can scarcely contain ourselves. These facts show us the essential distinction and relation of desire and emotion. In the latter the inhibition is markedly within, there is simply a preparing for definite reaction; in the former the preparing has passed into preparedness, but to the preparedness there is some outer inhibition which prevents discharge.

Following along the lines marked out in emotion, we shall pass on to an analysis of desire.

(1) In desire, as in emotion, there is a content object. In this case, as in the other, the content is dynamically built up by our reaction upon some stimulus, and represents our intellectual valuation or interpretation of the situation. This situation, as interpreted by us, is what we want, and our interpretation viewed from the intellectual side is the object as it is for us. Here, too, the old law of relativity reigns. What is desirable to one person is not desirable to another. There are great variations. In fact, here, as in emotion, we see the expression of the inner character or nature of each individual. To one material things alone have value and are desirable; to another the great centre of life may be in the realm of art. In presence of these the soul rises up in all its power and seems striving to burst the limits imposed upon it. Here, also, we find two great lines of cleavage. Those things which appear to fit into the life of the individual, as expanding or enlarging, become goods to be sought. On the other hand, things which have a sinister import, which would cause contraction or suppression of our life, in whole or in part, become aspects of aversion—we loathe them.

(2) Looking inward again, we find an attitude as in emotion. But as we have seen, it is an attitude harmonious in itself, so that the various elements fall together in one outgoing stream. Reflexly we pour out toward or against the situation. Those things which have become objects of desire we are immediately impelled toward. In aversion we cannot help but be conscious of repelling the object—it has become one to be got out of the way, and at once we seek to relieve ourselves of

its presence. Here again the two great lines of discharge are seen and it is also to be noted that, as in emotion, our immediate attitude toward an object goes to make it what it is for us. Without using more detail we shall pass to the third element.

(3) We shall term this, as the same aspect in emotion has been termed, the 'feel.' In desire, as notably as in emotion, our inner springs are opened up and pour forth in tumultuous fashion. The more the reaction is hindered and we are shut off from some object dear to us, or something hateful is forced upon us, the more our nature surges and boils until it sometimes seems as if all barriers would be burst and we could contain ourselves no longer. Also, as this storm of feeling boils up and rolls in upon us, the intellectual valuation is enhanced, the more powerful becomes the strain upon our reaction. The desire as a whole is increased and rapidly passes on to its climax.

We now turn to (4) the color-tone of desire. As connected with the striving or straining this is always painful, but as regards the suggestion of satisfaction in the object and the impulse thereto it is pleasurable. Pleasure attaches to the prospective side and pain to the present. On the prospective side pleasure attaches to that which fits in with the life, sustaining and expanding it, and also to the suggested state of freed life when the contracting or damaging object of aversion is removed. Summing up these, we may say here as elsewhere that pleasure as color-tone indicates that which ideally or organically makes or appears to make for the expansion of life, while pain attaches to that which appears to contract or destroy our life in any department.

In emotion it was noted that the upheaval called forth active attention. So here we find that the craving of desire or the impulsion of aversion calls out our attention strongly, so that slowly or rapidly we seek to know what to do. This state is often one quite perturbed but nevertheless in its nature it is one of deliberation. When our minds are made up and we determine to act, the desire passes over into volition in which for the time being our keenest interest is centred. As our whole self, reflex and active, is engaged or

absorbed in the gaining of our end, the state is pre-eminently one of interest. True, in so far as one cannot attain one's end at once, desire remains and crops out in full force whenever our attention reverts to it particularly; but in so far as we are bound up in our action, interest is at its maximum. Where this is so, even when our action is directed to the overcoming of some hateful thing, we find the keenest pleasure. When our action is paralyzed and nothing can be done the color-tone becomes painful in the extreme. Free unimpeded action is interesting and pleasurable—impeded action or lack of it is painful and disinteresting.

Something farther is to be said. If our action is directed simply to one end it soon becomes monotonous. On the other hand the more we exercise in regard to anything the more interesting it becomes. Some line of action which perhaps was not very interesting at first but which has become set in our lives, connected and bound up with all that makes life worthful to us, shows itself to be a matter of deepest interest. The momentary interests pass over into the deep life interests, and, as such, constitute those things upon which we habitually react.

In gathering up what has been said we notice that those things which in any way come within the realm of our well-being, become matters of interest to us positively or negatively. Momentary interests, if continually reacted upon, pass gradually out of the immediate focus of the attention and become set in our nature. As they become set they become most interesting. When some new element of experience appears and it cannot be immediately assimilated but sets up different modes of reaction not yet harmonized within themselves, we have emotion. When the reaction immediately arises and is harmonized within itself but is inhibited in discharging, we have desire. These when attended to pass over into the immediate interest of action and although for a time vibrating from one to another, emotion tends to pass over into desire, which is interest inhibited. When inhibition is removed and the set reaction pours forth and calls out the active attention we have deepest interest.

Fundamental to all as the beginning and end we have interest immediate, or life. Also the two great courses of cleavage are seen to lie along the lines which make for the contraction or expansion of life. Wherever there is contraction of life the color-tone is pain; where expansion, pleasure.

REACTION TIME WITH REFERENCE TO RACE.

BY R. MEADE BACHE.

The fact of the coördinated existence to common observation of the apparently completed, final man, obscures in the minds of the multitude the rationale of his muscular movements. It is generally believed that in health, every one of those movements, whether in waking or sleeping, is derived from an act of either self-conscious or semi-self-conscious will. But physiology proves that some movements are simply reflex, as when, for instance, the hand may be said to draw itself away from a burn, and that others, although secondarily reflex, are still purely automatic, as when a child, having learned to walk, can walk thereafter without other self-consciousness than that necessary to start the machinery of walking; and of course every one knows that the vital movements, such as the beating of the heart and the processes of digestion, go on entirely irrespective of self-consciousness and will. Deep down in the physical constitution of man, graduated to his present condition through successively higher and higher types, with corresponding advance in structure and function, lies plain evidence of the derivation of certain contradistinguished movements, namely, automatic as contrasted with volitional movements. As the skull itself was, as discovered by Goethe, derived from upper vertebræ, it needs no demonstration to prove that, in the preceding period, there was no brain; and as all animals now provided with crania must then, nevertheless, have lived and moved and had their being, it also stands to reason that will, which has its organic seat in the brain, could have had no existence in that preceding period.

What, then, in one era of that primordial time, representing millions upon millions of years ago, constituted animal life? What indeed in some of the present forms of life, as

in the case of the simplest, the amœba, entitles them, as little protoplasmic masses, to be regarded as possessing animal life? Assimilation of food in a way analogous to digestion, and with a difference from vegetable life,—through a law almost universal even in the misty borderland between the lowest forms of animal and vegetable life,—the imbibition of nutriment in higher chemical combination than vegetable life can use it. So, also, in some of the past history of incipient man, he, too, was a creature destitute of capacity for the designed taking of food and direction of energy, destitute of any capacity for movement except that which was purely reflex, not purposive. It follows, as proved by biology, anatomy, and physiology, working hand in hand, that man having been evolved from successive forms which, at the beginning and long afterwards, were reflex in their movements, must continue, in harmony with his present environment, to be so endowed. Development depends upon natural selection and functional use, and these are in turn dependent upon environment, and man's environment has not so changed as to enable him to dispense with reflex, and secondary-reflex, combined in automatic movements.

The foundation of man's earthly existence is and was what Huxley terms 'the physical basis of life,' protoplasm; and now, in the highest estate which he has reached, metabolism of that basis, the chemical building up to higher forms and the breaking down to lower forms of protoplasm, represents the varying intensity with which he lives. So varied in its conditions and consequent manifestations is this physical basis of life that Dr. Michael Foster writes in the article 'Physiology,' in the Encyclopædia Britannica, "the protoplasm of one muscle must differ from that of another muscle in the same kind of animal, and the protoplasm of Smith's biceps must differ from that of Jones's." Biologists and physiologists do not deny to protoplasm, even in its simplest forms, the quality of consciousness. If they did, it would be impossible to draw the line where consciousness begins in one form of life and where it ends in another. In a certain broad, intelligible sense, it may be said generally, that where we see life of even the lowest form assimilating

food of a certain chemical constitution, there is animal existence and consciousness. There are exceptions in plant life, but they are few. But the consciousness referred to is not the kind that is covered by the term 'self-consciousness,' or by another term that is used to mark the distinction—'awareness.' By way of illustration of the difference, it may be said that the eye may be open and a picture of surrounding objects necessarily on the retina, but yet the mind may take no cognizance of the picture: the picture must be seen, but it may not be perceived. So also, in the lower protoplasmic life, there is consciousness for the requirements of mere being, but not 'awareness' of being and of its manifestations.

As, at the remote period indicated, in which millions upon millions of years are involved, man having no skull, and therefore no capacity of 'awareness,' his functions were then only reflex. Graduated beyond that point, he yet, in correspondence with his acquired vertebrate formation, became possessed of nervous structure serving the needs of his advancing form of life. If the being from which he was derived had no skull, it had neither cerebrum, cerebellum, pons, nor medulla oblongata, all of which are contained within the skull. He must at one time have had only a spinal cord, the present structure of which makes it a nerve centre as well as a conductor of nervous impressions. Therefore, in the being which was to become man, the spinal cord, which now represents the nervous agency of voluntary movements and tactile impressions must, as it was not dominated by will through the presence of brain, have been the seat of mere vital impressions and reflex action unaccompanied by perception. An animal, the amphioxus, the lowest of the vertebrates, still extant, has no head, but merely a vertebral column. The condition of man differs essentially now from that of his past. In addition to the spinal cord's being now more highly differentiated, it may also now be dominated by the will, through the organ of the brain, and it generally is, even in a measure during the incoördination of the nervous system during sleep, for the sense of existence and of personal identity is never lost even in dreams.

Endowed as man now additionally is, he consists of two physical beings, one of which, automatic, may or may not at times be dominated by the other, the intellectual, gifted with perception, intention, and will. He is, moreover, so organized now, and must so remain as long as the requirements of his present environment endure, as to bring it about that the dominant brain can give general, instead of particular, instructions to its automatic slave, which the latter will faithfully carry out to the extent of its physical ability. The automatic man is the educated slave of the brain, as proved by the fact that the art of walking, as well as all other complex actions, had to be acquired through the expenditure of a certain amount of instruction, attention, effort, and time. Walking is a complex muscular performance in which the man wills that his body shall walk, and leaves to his automatic part the execution of the task. Having been once acquired, the ability has become and remains purely automatic, and whatever may be said of walking applies with equal force to any other complex muscular movement of man. One should not suppose that when an athlete is striking the punching bag of a gymnasium with the utmost rapidity of which he is by training capable, that each blow emanates from a special act of will. If that were so, each blow would show the 'reaction time' of the man; that is, the interval between perception and action. But this is obviously not so, for the number of blows, dividing the time in which they are struck, proves that intermediate perception between every two is eliminated. When, for instance, Corbett, the boxer, stands in profile and strikes the bag as rapidly as possible, the play of his forearms resolves itself into a blur, in which their outlines are scarcely visible. In striking the punching bag, perception for each action represented by a blow is discarded. The will determines that the blows shall be delivered, that they shall be delivered with a certain rapidity, and it continues throughout the operation to supervise their delivery, but it cannot supervise each, any more than it can determine their speed, which necessarily depends upon the automatic excellence of the instrument with which it is dealing. The will, which means simply the mind

resolved into action, has, in the case under consideration, nothing to do with the matter but to start, to preside over the action, to modify, and to stop it.

The preceding statements of fact bring us face to face with an important conclusion to be drawn which entirely differs from popular conception of the subject. Herbert Spencer somewhere calls attention to the contrast between the savage and the civilized man, in the circumstance that the former is so much more than the latter a creature of secondary reflex movements, and he illustrates this by remarking that, if a savage hurts his foot against a stone, the likeliest immediate response on his part is to kick the stone; an action indicating a development far inferior to that of a civilized, not to say an intellectual man. Now, the popular notion is, that the higher the intelligence of a man, the more immediately responsive his movements must be to stimulus. But we have already seen reason to believe that, all educated movements being automatic, it is the lower, and not the higher man, who should be more responsive to stimuli of the sort which are related to secondary reflex action, that men, in proportion to their intellectuality, should tend less and less to quickness of response in the automatic sphere, that the reflective man should be the slower being. That this is so I have for a long while believed, and I find to my mind a sufficient reason for its so being in the fact that the automatic preceded the intellectual condition of man, and that, with the decline of his primal rude life, secondary reflex movements should have become in lesser and lesser degree a necessity for his self-preservation. He should have discarded, I thought, in proportion to his intellectual advance, whatever was becoming less and less useful to him in his changed environment. In all evolution is modified or discarded whatever there is of lessening or no requirement for life under new conditions.

The popular notion that the more highly organized a human being is, the quicker ought to be the response to stimuli, is true only of the sphere of higher thought, not at all of that of auditory, visual, or tactile impressions, which invite secondary reflex action. As here stated, response to

such stimuli, not depending upon the more highly organized, but upon the less highly organized portion of the nervous system, the most ordinary intelligence should suffice for its exercise; and in proportion to intellectual advancement, there should be, through the law of compensation, a waning in the efficiency of the automatism of the individual. It has been contended, as an unanswerable argument, by a crucial test, that other things being apparently equal, high intelligence in one man as compared with another would result in the favorable issue to him of a pugilistic contest in which he might be engaged with that other. But here is introduced an element which is not necessarily involved in consideration of the 'reaction time' of two of the kind of men usually engaged in such contests. The answer, therefore, is that, other things being equal, relatively greater intelligence should give its possessor the victory, but only on the condition that the intelligence is superior, but not high, for it does not require high intelligence to conduct a pugilistic contest; while, on the other hand, inasmuch as the intelligence requisite for the conduct of a pugilistic contest is at best low, if one of the combatants, otherwise apparently equal, be an intellectual man, that is, has intelligence far beyond the purpose, and the other has nothing but intelligence sufficient, the former would be handicapped by his lesser relative automatic excellence, lost perforce of his intellectuality. His intellectuality having been gained at the expense of his automatic capacity, he would be defeated by the man whose lower, but sufficient, intelligence had subtracted less from his primitive constitution. The law of compensation is binding, and declares that growth in one direction of correlated structure and function involves diminution in another, and here we have a case of distinctly correlated structure and function. In a word, the automatic superiority of the less intellectual man being greater as such than that of the other, and his intelligence quite equal to the purpose of pugilism, he would win in a pugilistic contest. If it were otherwise, then the theory here brought forward, as supported by observation, and by experiment remaining to be finally presented, would fall to the ground.

Pride of race obscures the view of the white with reference to the relative automatic quickness of the negro. That the negro is, in the truest sense, a race inferior to that of the white can be proved by many facts, and among these by the quickness of his automatic movements as compared with those of the white. Many men, however, resent any claim for him of superiority, even in the low sphere of automatic movements, notwithstanding that there are several negroes and mulattoes at the present day in the ring whose excellence is scarcely approached, some of whom have often cheerfully encountered opponents of much greater size and weight for the privilege of being able to prove their skill. When additionally it is considered that the negro has in pugilism the advantage over the white in length of arm and thickness of skull, it ought easily to be seen that, with equal opportunity, were prejudice not so strongly against him, he would be regarded as the boxer *par excellence* of the world. It would be vain to say that Corbett is as quick as, or quicker than, any negro boxer. He may be quicker than any present negro boxer, but even that is doubtful. It is, however, contrary to all scientific practice to generalize from the case of a single or even of a few individuals by way of establishing a law. It is relative race characteristics of which there is now question, as previously there has been question of the relation between different individuals of the same race. Any one who will dispassionately observe any group of skylarking whites, and compare them with a group of negroes under the same circumstances, would be forced to admit that the latter are quicker in their movements; that the negro is, in brief, more of an automaton than the white man is. When bluff John L. Sullivan declared of the colored boxer, Jackson, that he would not fight him because of his race, he probably builded better than he knew when using the word superiority in the sense not related at all to a pugilistic contest.

Having, from observation, for a long while believed the fact to be as here stated, with reference to the relative automatic excellence of individuals of lower races as compared with those of higher ones, and having additionally ascribed

the fact, if it be a fact, to the cause mentioned, I finally determined to submit the matter to the test of experiment. With magneto-electric apparatus, now so common and easily adapted to various investigations of the sort, Professor Lightner Witmer, of the University of Pennsylvania, has at my suggestion made a number of experiments for determining the reaction time of Whites, Indians, and Africans, with the results as given below. The reaction time of women, as settled by the same indisputable method, was long since determined as less than that of men, and this result, it will be observed, is in strict accordance with the fact that the brain development of men, as compared with that of women, is greater, even when taking into account the relatively greater weight of normal individuals of the male sex as compared with that of normal individuals of the opposite one.

Although I do not, in contradiction of my own statement, mean to imply from the few experiments here presented, that they should be regarded as conclusive of the views here expressed, yet I present them for what they are numerically worth, with the intention to increase their number, and in the hope that, from the fact of their presentation, other persons will be led to follow the same line of investigation.

CAUCASIAN RACE.

Different Persons.	Age.	Auditory.		Visual.		Electric Shock.	
		Mean of 10 Observations.	Mean Variation.	Mean of 10 Observations.	Mean Variation.	Mean of 10 Observations.	Mean Variation.
1. . . .	22	135	7.0	152	10.0	141	4.0
2. . . .	24	130	7.0	140	8.0	128	11.0
3. . . .	16	141	13.0	174	10.0	187	9.0
4. . . .	14	132	8.0	159	10.0	138	3.0
5. . . .	15	182	20.0	214	6.0	142	14.0
6. . . .	19	147	19.0	164	11.0	119	13.0
7. . . .	18	139	12.0	155	22.0	95	11.0
8. . . .	19	170	15.0	191	12.0	150	27.0
9. . . .	29	123	6.0	164	9.0	121	7.0
10. . . .	15	234	17.0	201	12.0	229	15.0*
11. . . .	24	119	7.8	118	3.0	103	6.7
12. . . .	15	111	12.1	145	3.9	133	6.8
Final Means,	19	146.92	12.0	164.75	9.7	136.33	10.6

* In all the tables the figures represent thousandths sec. Compare times in this line by all three tests. They are abnormally slow.

INDIAN RACE.

Different Persons.	Age.	Auditory.		Visual.		Electric Shock.	
		Mean of 10 Observations.	Mean Variation.	Mean of 10 Observations.	Mean Variation.	Mean of 10 Observations.	Mean Variation.
1	18	165	5.7	168	8.5	152	3.5
2	21	115	5.5	121	3.9	100	3.4
3	14	128	5.4	148	6.2	118	2.5
4	23	144	6.1	127	3.1	122	3.6
5	14	70	6.2	119	4.8	94	5.3*
6	16	104	11.0	139	9.9	121	5.4
7	16	109	10.1	151	6.3	123	2.4
8	17	107	10.6	120	6.2	90	3.9
9	17	120	13.0	141	6.9	120	8.2
10	18	117	12.4	141	7.7	110	5.8
11	19	100	5.3	118	3.7	114	4.6
Final Means,	17½	116.27	7.7	135.73	6.1	114.55	4.4

*Pure blood Indian. Abnormally quick.

AFRICAN RACE.

Different Persons.	Age.	Auditory.		Visual.		Electric Shock.	
		Mean of 10 Observations.	Mean Variation.	Mean of 10 Observations.	Mean Variation.	Mean of 10 Observations.	Mean Variation.
1	16	114	7.2	157	8.4	107	10.3
2	19	113	10.4	148	14.2	108	5.4
3	19	127	7.7	131	4.6	100	3.6
4	20	125	5.7	138	6.9	120	6.0
5	19	164	24.7	173	7.0	137	13.9
6	22	164	13.4	187	10.7	178	8.7
7	26	121	13.8	118	11.8	103	5.0
8	34	148	4.0	159	5.9	141	7.7
9	38	109	4.8	165	11.2	118	6.5
10	16	120	6.0	162	8.0	112	5.0
11	25	126	5.0	144	7.0	128	8.0
Final Means,	23	130.00	9.3	152.91	8.7	122.91	7.3

The first thing that strikes one, upon examination of the tables, is the relative slowness of the Whites, as compared with the Indians and Africans. This is in accordance with the theory. But what is not in accordance with it, is that the reaction time of the Indians is shown by the tables to be less than that of the Africans, and the African is not so high in race as is the American Indian. It is possible, however, that the eventual explanation of this, when enough observations shall have been secured to demonstrate a law, will be that the Indian belongs to a race which for centuries cultivated quickness of movement as a necessity of his existence. Besides, the so-called Africans on the list have a larger intermixture of white blood in their veins than have the Indians on the corresponding list. It would seem, however, that the largest factor, as a disturbing element, is derived from the circumstance that the African, of the class here referred to, of whatever infusion of white blood in his veins, inherits physiological effects from generations of slavery. It must be, if we can ascribe to the Indian, through the influence of heredity, an extraordinary low reaction time, that we should admit, through heredity, the effect of converse conditions to which the African has been subjected. Whoever has seen slaves hoeing, in their listless fashion, in a cotton-field, or engaged in other forms of labor, must feel well assured that the mental attitude thereby betrayed could not fail in the course of generations to modify physical function. In sum, the conclusion must be, so far as the tables may elucidate the subject, that the African is quicker than the White, despite his hereditary history, and the Indian is quicker than both, perforce of his hereditary history.

I wish to call attention to a strange detail, to the case of No. 5, on the list of Indians. That case happened to be one of a full-blooded Indian, and as is seen, his reaction time is marvellously low. If 70 had appeared alone as the result of the auditory test, it would be justifiable to discard the observation, but the auditory, visual, and tactile tests all correspond, in due relation to each other, and therefore it is impossible to regard this as any other than an exceptional case of quickness even amongst Indians. It is interesting to

contrast with this the case of No. 10, amongst the Whites, with reaction time about three times slower than the reaction time of the Indian No. 5. Here again we perceive, as in the case of Indian No. 5, that the times, as determined by the auditory, visual, and tactile tests correspond perfectly, and that we must regard this as a case of abnormal slowness of reaction time even among Whites.

In the list of Whites there are twelve individuals, and in the list of Indians, eleven, but only ten in the list of Africans. But, then, it must be considered, that in each of the first two lists mentioned is included an abnormal case,—one of slowness and one of quickness. It would take more than one or two additional cases to produce an entirely satisfactory mean. To obtain perfectly satisfactory final means it will be necessary, of course, to make many more observations, and these I hope eventually to secure.

The views which I have here expressed I had entertained, from observation, for very many years, long before I suspected the scientific bearing which they have. I never found any one, however, to whom I communicated them who seemed to recognize their probable truth, and it was at the beginning, and for a long period afterwards, impossible to prove the correctness of my position until the creation of electrical physiological apparatus enabled any one to put to a crucial test any such theory as is here presented. When at last the apparatus was invented, and the convenience came to me in the facility afforded by Dr. Witmer, I availed myself of the opportunity. The article which I here present was written several months ago, while the experiments at the University were proceeding. I had intended to publish it at once, and let the experiments follow, but upon reflection, I concluded to postpone its publication until it was in my power to give something that would at least point in the direction of the truth of my hypothesis, for otherwise, it might be received with entire incredulity. Now that I am able to present matter, which certainly does point, if it does no more than point, in the direction indicated, I do not hesitate any longer to publish what I have held back.

It only remains to add, for the benefit of the general reader, that the record, as represented in the tables, is made in thousandths of a second, as registered by the electro-magnetic physiological apparatus. In the auditory test, the subject, upon hearing the prescribed short sound, releases a telegraphic key upon which his finger is resting. The difference of time between the sound as it takes place and the release of the key is recorded by the apparatus. In the visual test, a long pendulum is suspended away from the perpendicular in a room adjoining that in which the subject sits. The subject releases the telegraphic key at the moment when he sees a flash of light given by the pendulum-bob passing a small opening in the room where he is placed. The difference of time between the actual passage of the bob and the time when the telegraphic key is released is recorded by the apparatus. In the tactile test, a slight electric shock is given to the wrist of the subject. The difference of time between the shock and the removal of the hand from the telegraphic key is recorded by the apparatus.

DISCUSSION.

PAIN NERVES.

In the July number of this Review appeared an article entitled 'The Psychology of Pain,' by Professor Strong, the same having been read by him at the late meeting of the American Psychological Association at Princeton. The paper presents two main conclusions, with one of which I am in such hearty accord that I may the more freely express a small doubt about the other. This other is, in Professor Strong's words, "that the evidence seems on the whole to indicate that pain impulses are exaggerations of tactile, heat and cold impulses, and are conducted inward by the same fibres."

Presuming that Professor Strong presents in his paper the strongest evidence he knows of for his opinion, it would appear that he rests it chiefly on the cases of locomotor ataxia mentioned by him on pp. 336 and 337. The point he there brings out is, the presence of hyperalgesia to temperature within the same areas which are analgesic to touch.

Professor Strong deduces from this that "temperature pains are more closely bound up with normal sensations of temperature than would be the case if pain impulses were conducted by a special set of fibres." It is against the legitimacy of this deduction that I beg leave to raise a question.

These cases of locomotor ataxia, as do others, certainly show the seat of the disease to be puzzlingly specific. Apparently, in the same section of the cord, the touch fibres are left intact, while the temperature fibres from the same cutaneous area are affected. The reverse also frequently happens. Or again, in some cases, even touch and cold are left, with the loss alone of heat. But since, beyond doubt, the disease is thus mysteriously specific, why does Professor Strong refuse to conceive that it may likewise extend to the affection of specific nerves of pain? Granted that ataxia sometimes affects all the cutaneous impulses simultaneously, should one therefore conclude that all of them are carried in one fibre? Or with just heat and cold affected, does that necessitate their impulses running in the same fibre? If not why are we to conclude,

on precisely similar evidence alone, that heat and heat-pains must be carried in the same fibres? It would seem to me that only those cases would be in evidence for or against the separateness of pain and other fibres, which showed pain surviving with loss of corresponding other sensations. And since Professor Strong, from the reports he quotes from Dr. Mitchell and others, appears to be abundantly aware of this not uncommon occurrence, one is a little perplexed at his summary rejection of separate pain nerves.

Going further, we find plausible reasons why touch, cold and heat fibres should respectively be bound up each with its own separate pain fibres—granting that these exist. The matter of end-organs suggests this. Though we know little about them, it is fair to suppose that the same outer impulses demand similar end-organs for the correspondent pain nerves, as for the nerves of the ordinary sense; and that different end-organs are demanded for different sorts of pain stimuli. Under this view we should expect the heat and the heat-pain fibres to have similar, if not indeed identical end-organs. And if so we should not be surprised to find these fibres more closely bound together in the cord than are those of touch and heat, which, probably, have very different end-organs seated in different dermal layers.

In view of the overwhelming evidence, both normal and pathologic, of pain from all sorts of stimuli, unaccompanied by other sensations—from temperature as well as from other—we might leave here the theory of separate pain-nerves to stand on its own merit, did not one other point in Professor Strong's unique exposition of his subject call for comment. While rejecting the old 'quale theory,' he still clings to the notion that "pain impulses are exaggerated tactile, heat and cold impulses, and are carried inward by the same fibres." Of course he does not now mean, as did the old traditional pain-pleasure theorists, that heat, for example, has one ordinary form of impulse, and heat-pain another exaggerated form all to itself. For since, unmistakably, we experience heat and heat-pains simultaneously, such a notion would leave us conjecturing how two forms of impulse, a weak and a strong one, should be induced by the same stimulus and carried in the same fibres at one and the same instant. What Professor Strong undoubtedly means is that the exaggerated impulses are as much tactile, heat and cold impulses as they are pain impulses; and that when we do experience pain both it and its accompanying sensation rises from the same impulse, only that we do not have pain unless this impulse is of the exaggerated form. To explain, under such a conception, how the two sorts of

sensation should result from precisely the same exaggerated impulse, yet be two separate sensations, Professor Strong avowedly falls back upon Wundt's 'shunt' theory,—which is plainly the only one in the field at all reconcilable with his peculiar views. But since Professor Strong himself suggests certain objections against Wundt's theory, and mentions no objections whatever against the theory of separate pain-nerves except, as may be inferred, that they have never been visibly demonstrated, we are at some loss to understand his unqualified acceptance of the former in face of many obstacles, and his as unqualified rejection of the latter in face of no obstacles.

The objections to Wundt's theory may be summarized as follows: First, the numerous cases, normal and pathologic, of pain without accompanying sensations of touch, heat or cold. Next its demand for a much more complicated and duplex arrangement of our sensory nervous systems—cranial as well as cord—than present anatomy gives any suggestion of. And finally the objection that it demands hypothetical complications for which there is no need whatever—all the phenomena being much more reasonably explained by the very simple theory of pain-nerves.

These are formidable objections to the only theory that is reconcilable with Professor Strong's summary of present evidence. On the other hand, the sole evidence, as we have said, which he brings against pain-nerves, is the fact, apparently, of their not having been objectively demonstrated. But without pressing Professor Strong to explain in what respect Wundt's 'shunt routes' have been *more* objectively demonstrated than pain-nerves, I will ask what sort of demonstration of pain-nerves he can ever reasonably expect, even granting them actual existence? Suppose he actually saw two fibres ending in a touch corpuscle—one a fibre of pain and the other of touch—would he expect them to look different? And since, according to his own view, they are stimulated by the same cause, and, as I have suggested, very likely by the same end-organ, is it likely that he would be able to distinguish the two by direct normal experimentation?

In so far as I can see, therefore, the best possible evidence we can well hope for with reference to pain-nerves is precisely that which we now have. Namely, the frequent occurrence, normally and pathologically, both of pain without other sensations, and of other sensations without pain; and above all, the entire harmony of the doctrine with the remaining facts of neurology, both existent and genetic. As against this, the unceasing struggle of modern

psychologists to patch up the old 'combination theory' in the face of overwhelming difficulties, and with nothing whatever to recommend it save the 'hoary respectability of tradition' is surely a remarkable exhibition of the vitality of custom. By way of rousing Psychology from this slavishness, I am inclined to endorse the emphatic words of Professor James; namely, that we have in the traditional pain-pleasure theory "*one of the most artificial and scholastic of the untruths which disfigure our science.*"[1] A great step is gained, however, by Professor Strong rejecting one-half of this monstrosity. So bisected, the other half is likely to die very decently.

HERBERT NICHOLS.

PROFESSOR WATSON ON REALITY AND TIME.

In a recent interesting article, Professor Watson aims to clear up the relation of time to the absolute.[2] He devotes most of his space to preliminary considerations in psychology and to the examination of Bradley's conception of reality and of McTaggart's recent exposition of Hegel's doctrines of the absolute and of time. Professor Watson's own view of the time-process in its relation to the absolute is stated only as it is implicated in these criticisms—the positive treatment being reserved for a later article. The conclusion which he thus announces is in these words: "An Absolute which manifests itself in the time-process, and yet is self-complete." This view, however, must not be considered as the traditional 'reality-behind-appearance-view' of the transcendentalists, as Professor Watson is at pains to say: it is much nearer, as the present writer understands it from the partial statements of Mr. Watson, to the later view of Lotze contained in the *Metaphysic* (as contrasted with the Lotze of the *Dictaten*). This may be made plainer by further quotations. Professor Watson says: "If the Absolute is self-complete apart from the time-process, it cannot be manifested in that process: if it is manifested in the time-process, whether it is self-complete or not, at least it cannot be self-complete apart from the time-process, but the time-process is essential to its self-completeness." "We reject as self-contradictory the conception of the Absolute as self-complete apart from the time-process." Lotze's view, with all its ins and outs, is well presented by Falckenberg in his recent articles: and the pondering of his views, especially the distinction whereby he finds

[1] PSY. REV., Sept. 1894, p. 525, note.

[2] *The Absolute and the Time-process. Philos. Review*, July, 1895, pp. 353 ff.

succession necessary to an Absolute which is changing reality, while duration can not be so considered, leads us to see that his problem is very similar to that which Professor Watson is taking up, when he goes on to say "we are immediately confronted by the difficulty that a world that is in process does not seem to be self-complete." It may not be fair to anticipate that Professor Watson's solution will finally be similar to Caird's: and it is difficult to see how he can finally get an Absolute which will be free from the charge of being 'static.' But there are indications in this article that Professor Watson, who has the just reputation of being one of the very ablest of the 'Intellectual Idealists,' is going to work that kind of thinking free from some of the weaknesses with which it has been beset in the eyes of those who are unable to find in the dynamic categories simply the 'telling-off' by us finites of a series of intellectual terms. What I mean by indications are these: Professor Watson in this article seems to recognize the need of some kind of an ontological construction of evolution—although there are indications, too, that he may fall back on the resource to be found in the subjectivism of the category of evolution (pp. 367f). Farther, Professor Watson shows a certain unexpected affiliation for Lotze, again, in essentially agreeing that the question of metaphysics is 'what reality is: not how it is made.' He says: "If it is asked *why* the Absolute reveals itself gradually in the finite, I should answer that the question is absurd: we can not go behind reality in order to explain why it is what it is: we can only state what its nature, as known to us, involves." Does not this seem to 'indicate' that there may be some further agreement toward a dynamic view of reality, in spite of Professor Watson's contention that reality must be self-complete in the sense that it is intellectually constructible? Then there is a third 'indication.' It is found in the good piece of psychology which Mr. Watson gives us in this article in treating of conception and judgment. This psychological digression is not new in its teachings: it is a series of views made very clear by the newer logicians. I myself developed the same views in the first edition of my *Senses and Intellect* in 1889. But the use which Professor Watson makes of the 'organization view,' as I may call it, of conception and judgment, is what I find interesting. I shall speak of his point against Bradley further along: here it is enough to point out that Mr. Watson finds reality a function of progressive mental organization—thus denying the very possibility of a construction of reality apart from this organization itself.

How then can the inference be avoided that the absolute arises as real by mental construction also? But we have no intellectual organization of which the untemporal, the logically self-complete, the undynamic, is a function. Professor Watson, it is true, avoids this issue, and contents himself with the old antithesis of the intellectualists—"The consciousness of the finite presupposes the consciousness of the infinite" (p. 368), and "we are compelled to regard all finite or dependent being as presupposing a self-determining principle" (p. 368). Why are we? I for one, am not. To be sure, if we make a logical antithesis with a supposititious finite, defined as dependent, at one pole, we must go on to put a supposititious infinite at the other pole: but it is going back to scholastic logic to say that either must then have reality, or gets it by this dialectic of terms. As a matter of fact, when I ask my consciousness for the mental organization which issues in the conception 'infinite', it is not there—and in my private view, neither is it there for the logical term finite; but that is by the way. If this be true, that there is no mental construction of any such object as the infinite or the absolute, how, on Mr. Watson's true psychology, can there be a function of it called its reality? Or is this the exception in the doctrine of reality which proves the rule?

In his preliminary determination of the Absolute, in the course of which the examination of Bradley occurs, Professor Watson makes good use of the 'organization view' of reality, as I have termed it for brevity sake. The aim of his criticism is to show 'that reality in its completeness must be a thinkable reality.' "If it is meant that there is in reality something which cannot be made the object of thought, because it is unthinkable, I do not see what kind of reality this can be" (or by an impertinent paraphrase, I can not think it!)

The argument is forcible, and but for certain criticisms of limitation, in my opinion valid. It runs thus: If reality is, as a true psychology teaches, nothing apart from the mental construction or content itself which is said to be real, then there can be no room for Bradley's contention that the knowing or judging process always vitiates reality, inasmuch as it issues in a series of partial predications, none of which adequately expresses reality, and which are in the main contradictory among themselves. This is, it will be remembered, the road which Bradley takes to show that all knowledge is appearance or pure *Schein*. Now, says Watson, such a reality apart from the organized content of knowledge is quite supposititious: the very meaning of reality is psychologically just this mental organ-

ization, at the different stages of it secured by progressive conception and judgment. So Bradley's distinction between that which would be real if we could get hold of it, and that which is not real because we have got hold of it, is throughout a false distinction. This criticism is valid, I think, as against Bradley's impeachment of judgment; but not valid as used by Professor Watson in his further positive contention that if this be so then reality must be capable of being thought, in whatever instance it be considered, and so in the instance of the absolute. True as far as it goes, this view of reality is yet inadequate psychologically, and proves in the sequel not only to leave other views open, but to allow a return to the essence of Bradley's contention. This I may take a little space to show.

A reading of the recent new-school Logics,—Sigwart, Bosanquet, Bradley, and above all the disciples of Brentano,—shows us that there is a partial agreement in regard to the predicate 'existence.' This agreement may be brought out in the light of the foregoing by saying that Professor Watson converts a proposition which is (1), not universally true and is (2), inconvertible. He says, in effect, all mental constructions give us at once and *ipso facto* what we mean by reality, hence all reality must be found in such possible mental constructions.

Taking the first member of the sentence first—it is not true as a universal. The Logics say differently; and this is just the value of the partial agreement they are effecting, as against the older interminable disputes as to whether existence added anything, when thought in connection with an object, to the mere thought of the object. The Logics say in answer to this question: No; the thought of existence adds nothing to the object as merely thought. And this is the valuable contention which Professor Watson enforces against Bradley. But the Logics then go on to say more: The thought of existence is a different psychological mode, nevertheless, and finds itself quite a different psychosis. The thought of a thing as existing has the mode, or is the psychosis, which we call *belief*. And whatever it is that constitutes this 'mode' different from that of the mere thought-content itself, it is a real difference which psychology must recognize. It is not all thought-constructions which carry the reality predicate. It is only some of them. Sigwart would say only those which are (necessarily) judged by us: Bosanquet seems to wish to say only those which carry some kind of necessity other than the necessity with which sensations break in upon us. But whatever the lines of distinction be, they must be lines drawn by something else than thought; since the content remains the same—to be believed

to be real or not—and existence is not a thought-predicate. The distinctions are really, in my opinion, distinctions of *attitude*, motived largely by differences of feeling.

Furthermore—to take up the second point—even if it were true that all mental constructions carried reality with them, yet such a proposition could not be converted. There is yet a simpler form of consciousness, a mode of dealing with content, which does not involve existence as a predicate, but which nevertheless suffices for our prevailing activities in the presence of realities. 'Reality-feeling,' as I have called it, precedes belief; and belief—the assertion of the reality predicate—gives a return of the 'reality-feeling' again after a transition period of doubt, hesitation, suspension of judgment. Without taking space for going into points made earlier[1] —for the added reason, also, that they may be individual to myself —I may be content to put the fact in evidence that it is only a part of the realities which we get that are thought-constructions: most of them are after all *felt*. For example, does not ethical appreciation always run ahead of scientific description?

If these points be true, how can we say that the ultimate must, in virtue of psychological deliverances, be capable of being exhausted in terms of thought?

It would seem to be a competent statement, if we should modify the sentence, 'reality in its completeness must be a thinkable reality,' of Professor Watson; and say: reality in its completeness can not be merely a thinkable reality; even though it be capable of being thought, it must have in it the quality of moving the possible thinker in the ways we call belief, ethical appreciation, &c.; and farther, it may be so simple a thing, to the consciousness in which we are supposing the appeal to do the thinking about it to be made, that it can not be thought at all, but rests in its own limpid immediacy. This would seem to be the conclusion from the appeal to psychology, if Professor Watson insists on making it: and such a simple 'given' would seem in a measure to justify Mr. Bradley's insight in calling it 'that' as opposed to the 'what.'

J. MARK BALDWIN.
PRINCETON.

[1] See my *Feeling and Will*, Chap. VII., and the article *Feeling, Belief and Judgment*, in *Mind*, July, 1892.

PSYCHOLOGICAL LITERATURE.

GENERAL.

An Essay Concerning Human Understanding. JOHN LOCKE. Collated and annotated with prolegomena, &c., by A. C. FRASER. Oxford, Clarendon Press: New York, Macmillans. 1894. Vol. I. Pp. CXL+535. Vol. II, pp. 421. Price $8.

In this book Professor Fraser has done the philosophical world a service which none of those interested will be slow to recognize. We now have for the first time a convenient text of one of the greatest of English classics: introduced by one who is so well versed in modern tendencies of thought as to be capable of emphasizing the salient ideas of the text: and published in a form which itself makes a commentary on the dignity and common-sense of John Locke, 'the plain man of plain ideas.' The introduction by Professor Fraser covers CXL pages, 'biographical, critical, and historical.' The expository and critical part seems to the present writer to have all the mèrit of the calm and judicious spirit, coupled with directness of style, which characterizes Professor Fraser's 'Berkeley,' and which serves to set Locke forth in a peculiarly favorable light. It is so different from the form of 'Introduction' begun in England by Green and carried forward by the others—Caird, Jones, &c.,—which begins by obscuring what seems plain, and then trying to teach a new form of obscurity by 'showing up' the other. It may be true, as one sometimes hears, that the best exposition always comes from the man whose own opinions are not 'urgent for the utterance,' and it may be that it is for this reason that Mr. Fraser sticks true to his text: but in that case we must still be glad that the right man took up the work and hope that he may long be spared to do more of it.

The main merit to which I refer is seen in the many points on which Locke was really innocent of an opinion of his own or ignorant further that he was raising a question. Professor Fraser is true to Locke in letting him stand there in all his unfinished and fragmentary conceptions. Note, for example, the remarks on Locke's doctrine of real existence as resting on 'irresistible assurance' (LXXXIIIff), and the general remarks on Locke's views of 'the idea of self,' &c.

(LXXXV). "The treatment of the subject," says the commentator, "in the *Essay*, shows his disposition to avoid speculative questions, and the ultimate mysteries, and to remain contented with the point of view that satisfies ordinary minds." Yet on this very question, when we come to look at Locke exactly as he was, we see that it is very difficult to find in the new logical treatises which are attempting to set a reality 'necessarily' given in every act of judgment, very much essential progress on the position of the 'plain man' of the seventeenth century. So in other connections, the value of the exposition is as much in setting the author's limits as in expounding his positive theories: for it is just what seem to be his limitations which may be most instructive to the historical student. Has not Locke himself exhorted us in commending Anthony Collins?—"You have a comprehensive knowledge of it, and do not stick in the incidents, which I find many people do."

There is one point of view taken by Professor Fraser in a more positive way, however, which is in line with one side of the discussions relative to Locke's place in the historical movement of later philosophy: the view that Locke's refutation of Malebranche is in turn a refutation of the modern attempt to father a Lockian paternity upon the form of idealism which arose upon the basis of the 'theory of ideas' (XLVIII). "All this sheds light," says Mr. Fraser, "on many passages in the *Essay* in its recognition of the ultimate incomprehensibility by us of our own finite and transitory perceptions, and of God's infinite knowledge; so that human philosophy can offer no theory of either, much less explain the one by means of the other."

The annotations throughout the two volumes, in the shape of foot-notes, show the same moderation and caution: and for that reason they do not contain much of value to the modern psychologist apart from the elucidation of Locke. Possibly the psychologist who gives them the more detailed examination will be more inclined than the present writer to say that they reflect inadequate information in respect to recent phases of psychological thought.

J. M. B.

Friedrich Edward Beneke. The Man and His Philosophy. An Introductory Study. F. B. Brandt. New York, Macmillan & Co., 1895. Pp. 167.

This is the fourth number of the Columbia College Contributions to Philosophy, Psychology, and Education. It is precisely what its title promises, an introductory study of Beneke and his philosophy,

concise, well arranged, and clear, that is, as clear as any exposition of Beneke's philosophy can be. As there is no other monograph in English on Beneke, and as he represents one distinct phase of the many sided development of the Kantian philosophy, the appearance of this book is timely. Indeed, Dr. Brandt contends that it is in Beneke alone that the true development of German philosophy after Kant is found.

Beneke died in 1854 at the age of 56, having spent nearly all his life as Professor at the University of Berlin, for the most part without salary.

His significance is chiefly psychological. Metaphysics and pedagogy represent applied psychology and received considerable attention. Psychology is to be treated strictly as a natural science; it is to depend wholly on experience and is to follow rigorously the method of the objective sciences. But the experience is to be 'inner experience,' that is, individual consciousness of our memories, imaginations, concepts, etc., as distinguished from 'outer experience,' that is, perception of things. But psychology is by no means to be a mere descriptive science of inner phenomena; in inner experience there is revealed to us the thing-in-itself, the very essence of the soul. To say that this is promising is mild, it is captivating. But when we are told furthermore that this natural science of psychology is free from materialism, that it escapes subjective idealism by giving us a knowledge of other human beings and bodies, that it establishes the immortality of the soul in the form of an immortal personality, it would seem to those in search of a system of philosophy that it would be foolish to look further. Such seekers, however, will be a little disappointed with the outcome. Lured by Beneke's promises of empiricism, we enter upon a long and minute psychological analysis, in which we are introduced to a whole company of elements, powers, processes and existences, with strange names and stranger faces, which put to blush the worst Herbartian 'jargon.' We are told of 'traces,' those unconscious psychical existences which become conscious memories, of 'movable elements,' which form the constituent parts of consciousness, and of primary faculties (*Urvermögeu*) and in Beneke's system these are as fundamental and necessary as Herbart's reals and his doctrine of self-conservation and arrest. The soul he defines as a 'concrete psychical organism,' of which we have absolute knowledge in and for itself.

The points of resemblance between Beneke and Herbart are many. Beneke was not, as he said, very much influenced by Herbart, but they were both inspired by the same Zeitgeist.

In his life Beneke was so unfortunate as to be in conflict with the 'court philosophy' of Hegel, for which he had no great liking. It is a suggestive commentary on Hegel and his times and the advance that philosophy has made since then, that whereas the students of those days, accustomed to the sky-wanderings of the master, turned away from the lectures of Beneke with the remark, 'This is nothing but sound common sense.' Students of our day turn away from him for the opposite reason.

However, every student of the history of German thought of this century must read Beneke and Dr. Brandt's introduction will be indispensable to English readers. The monograph has perhaps one fault—not a bad one. The author being himself apparently a very enthusiastic admirer of the system, exhibits occasionally a rather alarming disposition to force it upon the reader, even if, in Plato's phrase, he has to put it bodily into his soul.

UNIVERSITY OF IOWA. GEORGE T. W. PATRICK.

The Essentials of Logic: Being Ten Lectures on Judgment and Inference. B. BOSANQUET. New York, Macmillan & Co., 1895. Pp. 167. Price, $1.00.

To one who may be interested in the problems of modern logic, this book will prove to be of very great value. It forms a most satisfactory introduction to the general theory of logic. It contains the substance in brief of Bosanquet's larger work. The discussions of Bradley, Lotze, Sigwart and Venn can be far better understood and appreciated, after receiving the illumination and suggestion of these most interesting pages. The point of view of the modern logic is essentially and primarily an epistemological one. How do we acquire that knowledge of the external world which gives to us the fundamental elements of thought with which logic is concerned? As answers to this question, Bosanquet discusses three different positions. The first is that of common sense, which regards the external world as independent of our consciousness for practical purposes. Its activities and happenings proceed irrespective of any apprehension by our minds. The second is that of common sense theory, which in addition to the first mentioned position assumes, concerning the nature of this world, that it is apart from consciousness the same as it is for our consciousness. The third which he calls the philosophical theory, regards the objective as independent of our consciousness in the sense that it is what we are constrained to think in order to make our consciousness consistent with itself. This position is the one which Bosanquet himself holds. It seems

to me to be substantially the same position as that advanced by Sigwart, who assumes as a fundamental logical postulate that the given is necessary. In holding such a position one avoids the defects of subjective idealism; here the real, outside the mind, being inaccessible, falls away. In knowledge there is no passage from subjective to objective, but only a development of the objective. Knowledge, therefore, is the medium in which our world, *as an inter-related whole*, exists for us. Knowledge is always given in the form of a judgment. The theory of judgment becomes the basis of a theory of logic. The main portion of Bosanquet's work is taken up with an elaboration of the theory of judgment. He defines judgment as an affirmation, pronouncing the interpretation of our perceptions to form one system with the data of our perceptions. This interpretation, or enlargement claims necessity, or universality. It is concerned with reality in the sense that the real subject of every judgment is reality, and our world therefore as existing for us in the medium of knowledge consists, for us, of a standing affirmation about reality. Bosanquet's insistence upon the intimate relation of knowledge to reality in judgment, seems to us to be a most valuable and admirable defence of the true theory of logic. It follows too from such a position that the form of knowledge can not be considered wholly apart from its matter. The two mutually influence and determine each other. One of the essential functions of judgment is that of construction, that is, of exhibiting a whole in its parts, an identity in its differences, and in this it is both analytic and synthetic.

Bosanquet presents the types of judgment in an original manner according to the analogy of the development in plants and animals. The idea of a growth in the forms of judgment had already been emphasized in his larger work, whose title contains this idea in the words, 'Logic, or the Morphology of Knowledge.' This growth is traced through the forms of the impersonal judgment, perceptive, individual, and the abstract, including hypothetical and disjunctive. Moreover Bosanquet considers judgment in its distinctive character as a claim to truth. Here we must distinguish between an idea as a psychical presentation, and as a reference to reality. The former is strictly a particular, a psychical image; the latter is both less and more than a psychical image. It contains less and stands for more. It contains only what is central and essential in the detail of each mental presentation, and therefore omits much; it has in consequence an abstract and universal significance. Judgment therefore is the reference of such a significant idea to a subject in reality by means of an identity of content between them. As regards the relation of

judgment to language, the most important features of Bosanquet's treatment are the two following:—that the sentence must be regarded as the unit, and that the copula is not so much a link between a subject and predicate, but as the whole judgment considered exclusively as a cohesion between parts of a complex idea. It is the 'grip' with which the parts of a single complex whole cohere with one another. This leads to a view of judgment radically different from the two opposed theories which regard judgment respectively as a comparison between two ideas, and as a comparison between two things. Bosanquet's theory is that judgment is always the analysis and synthesis of elements in some one thing or ideal content. He strictly maintains the unity of judgment, in which the ultimate subject is always reality, and that it is possible always to mass the whole judgment as a single predicate directly or indirectly true of reality. Regarding the distinction between categorical and abstract judgments, Bosanquet is clear and consistent. The categorical is really the perceptive judgment in which the bond with reality is direct or explicit; in the abstract judgments, the affirmation of reality is indirect. Underlying them is the implied categorical judgment. The hypothetical judgment is based upon a supposition which in turn must rest upon reality. In the disjunctive judgment, there is a combination of categorical and hypothetical, in which negation is itself rendered by the disjunction positively significant. Bosanquet's treatment of negation forms an introduction to the general theory of induction; it is necessary to understand the one in order fully to appreciate the other. Negation contains positive significance, always implicit, which is the basis upon which the negation rests. We deny in consequence of our ability to affirm something incompatible with that assertion which we deny. Bosanquet's theory of inference grows out of his theory of knowledge. Inference is possible because knowledge as the interpretation of our world forms one self-consistent system. Inference in all cases therefore presupposes a system, that is, a group of relations, or properties or things so held together by a common nature that you can judge from some of them what the others must be. In induction you complete the system from the parts; and in deduction, the system being known, suggests the parts and their relations. Induction, thus based upon scientific system avoids the defects of induction by simple enumeration. Its method is by negative instances as already mentioned in which your positive observation is ever confirmed by a negative which has positive value, and which is itself the converse by negation of the original positive observation. The impression

left by these lectures of Bosanquet is that logic is concerned with living thought, and not barren forms; that its sphere is not the ideal, but reality as the source and test of all truth; and that its results are not artificial, but practical and abounding in rich material content.

PRINCETON UNIVERSITY. JOHN GRIER HIBBEN.

Einleitung in die Philosophie. O. KÜLPE. Leipzig, S. Hirzel, 1895. Pp. 276. M. 4.

This Introduction may be described as a brief encyclopaedia of philosophical doctrines. Prof. Külpe criticises such works as those of Herbart and Paulsen on the ground that their writers were too much limited by personal opinions as to what Philosophy should be and therefore failed to do justice to many of the factors that go to make up this discipline in its true historic and general sense. He then tells us that it is his aim to give a broad view of all the tendencies of philosophical thought, both past and present, together with a critical estimate of their relative values. In order to make the treatment as objective as possible each discussion is based upon a historical summary including all the important phases of development in the given direction down to the most recent times. These summaries must, of course, be very much condensed and, though it is not to be disputed that they have the advantage claimed of giving a greater comprehensiveness to the work and a firmer foundation to the conclusions, yet it must be seriously questioned whether readers of an 'Introduction,' unfamiliar with the history of Philosophy, will be able to follow intelligently these brief outlines. In this connection we may notice the short lists that are given of the chief works on each subject. These will serve as valuable introductions to general philosophical literature.

Another characteristic of the book is the denial of the possibility of reaching a unitary system in Philosophy. History shows us that widely divergent views exist as to the problems and methods properly belonging to this field of investigation. This disagreement can be explained only by recognizing that the problems are really of a most heterogeneous character and can not be included under a single principle of classification, but must be arranged in distinct departments. We have accordingly to divide our subject into general philosophical disciplines, including Metaphysics, Theory of Knowledge, and Logic; and special philosophical disciplines, including Natural Philosophy, Psychology, Ethics, Æsthetics, Philosophy of Religion, and Philosophy of History. In order to gain a general view of the whole we must have first, a statement of the problems

arising in each of these departments, and second, some account of the general tendencies of thought that have arisen in the effort to answer these questions.

We take up then the problems of the philosophical disciplines. In brief Prof. Külpe's conclusions are as follows: Metaphysics has the mission of developing a theory of the universe which shall reconcile the practical demands of our moral and religious natures with the theoretical demands of our scientific investigations. The theory of knowledge inquires into the nature of the *content* of knowledge, dealing with such questions as the validity and limits of knowledge, the relation of subject and object, the division of knowledge into its material and formal elements, and the nature of general concepts. Logic, on the other hand, has to do with the *forms* of knowledge. It is the philosophy of the methods of thought and reasoning. Psychological logic and mathematical logic are not to be regarded as independent forms. The first is a part of general psychology and the second contributes no new facts but merely another form of expression. Natural philosophy is the discipline which stands in closest relation to the natural sciences. Its duty is the criticism of the presuppositions and methods of these sciences as well as an examination of the several concepts and theories which result from their investigations. Psychology in its earliest form was a science of the vital principle. At a later, and by no means unproductive stage, it was defined as the science of inner experience. The latest, and, according to our author, the true definition of psychology charges it with the investigation of experience in so far as this experience is conditioned by the subject, and since we know subjects only in connection with living bodies we must be more explicit and say in so far as this experience is conditioned by the living, embodied subject. "The chief problem that we must assign to scientific ethics as a special discipline is the collection and analysis of the prevailing moral standards of the day." Æsthetics can do no better than adopt the definition of Kant as philosophy of the beautiful on the one hand, and a philosophy of art on the other. Philosophy of religion is as yet a complex of historical and psychological investigations of religious phenomena. Nevertheless, as a discipline in which the fundamental concepts of theology are subjected to criticism it may hope for an independent development. Philosophy of history is the latest of all other special disciplines and can hardly lay claim to any definite program. It is evident that it will not have to do with the facts of history, but rather with the underlying concepts and methods of its study.

After the problems have been thus outlined we are in a position to take up the solutions that have been offered from time to time in the history of thought. These may be arranged in three groups namely :—Tendencies of metaphysics, of theory of knowledge, and ethics. These various directions of thought may be regarded as independent though not always mutually exclusive. Metaphysical tendencies may be divided into five classes. First, the number of fundamental principles employed gives us a basis for division into singularism and pluralism—this classification being prefered to one in which monism and dualism appear for the reason that these terms express qualitative rather than quantitative differences. Second, the quality of the fundamental principles—whether spiritual or material, causal or mechanical—gives us spiritualism, materialism, dualism and monism on the one hand, teleological and mechanical theories on the other. Third, the attitude towards the question of the nature of God has given rise to pantheism, theism, deism, and atheism. Fourth, a discussion of the freedom of the will has led to indeterminism and determinism. Finally, the psychological tendencies have arisen out of disagreements as to the nature of the soul and the character of psychical activity in the processes of knowing, resulting on the one hand, in substantialism and actualism, on the other, in intellectualism and voluntarism. Theories of knowledge divide on the question of origin into rationalism, empiricism, and criticism ; on the question of validity into dogmatism, skepticism, positivism and criticism ; and finally on the question of objective reality into idealism, realism and phænomenalism. Ethical tendencies may be classed into intuitionalism and empiricism in regard to their answers as to the origin of moral ideals; into emotional and rational. Ethics on the ground of their description of the nature of the motives to moral action ; into egoism and altruism in regard to the objects affected, and into eudæmonism and utilitarianism in regard to the object sought by moral action.

It will be impossible to follow the discussion into the details. Enough has been said to show the compass and plan of the book. A few of the author's personal views which appear in his criticisms may be of interest. Dualism is defended as the doctrine that agrees most satisfactorily with the special sciences as well as with the demands of the theory of knowledge. The criticism of materialism in this connection is particularly severe. The question of the nature of the soul is left without any positive settlement. The substance theory is vigorously defended against the attacks of modern actualism and yet this discussion ends with the statement that we are not

to regard this defence as a recognition of the validity of the substance theory, but rather as a proof that the impossibility of the existence of such a substance has not yet been demonstrated. It will be interesting to learn how Prof. Külpe's dualism will develope in this respect. Intellectualism and voluntarism are both rejected as onesided and the position is emphasized that all the elementary forms of psychical activity are to be regarded as on a par. On the question of the freedom of the will the negative position of determinism is defended.

The last chapter emphasizes the impossibility of a system in philosophy and defines the problems that this discipline has to deal with. These problems are three in number. First, the development of a theory of the universe. Second, the criticism of the presuppositions of the sciences as well as their resulting concepts and theories. Finally, philosophy has had historically, and still has the important duty of formulating new problems and methods which shall give rise to new special disciplines. C. H. JUDD.

LEIPSIG.

Æsthetic Principles. H. R. MARSHALL. New York and London, Macmillan & Co., 1895. Pp. X + 201.

The substance of this volume was given last winter as a course of lectures at Columbia College and subsequently prepared for publication. The author's aim is to present the outlines of his æsthetic theory freed from the psychological detail which accompanied it in his larger work on *Pain-Pleasure and Æsthetics.* The result is a little masterpiece of two hundred pages in which an unusually interesting and stimulating content is presented in a form which possesses many of the qualities of an English classic.

There are two parts to Mr. Marshall's discussion. In the first four chapters under the dual standpoints of the observer and art-producer, the author treats of the Field of Æsthetics; Pleasure and Pain, The Art-instinct and Æsthetic Standards. In the last two chapters under the caption of Algedonic Æsthetics, the negative and positive principles of æsthetics are developed. It is needless to say that the topics are handled with great originality and power. The same mastery of exposition, acuteness of criticism, keenness of analysis, fineness of psychological and æsthetic discrimination and sharpness of dialectical skill are conspicuous here as in the author's larger work, while his rich scholarship, although kept in the background, displays its abundant fruitage on every page.

Mr. Marshall's æsthetics rests on his psychological theory of Pleasure and Pain and it is here, I think, that the strength as well

as the weakness of his position is to be found. Professor James has said in connection with the author's larger work, that he has been more successful than any preceding writer in subsuming the phenomena of pleasure and pain under a single point of view. This is a merit which would not be seriously affected were it to be successfully shown that the point of view is not quite exhaustive. To such a criticism, I think, the theory is in fact, open. Pleasure and Pain are construed by the author as functions of the relation between the nerve supply and the draught that is made on it by the special activity with which it is connected, pleasure arising as the psychological effect of a surplus of nervous energy over and above the normal supply, while pain is the result of an over draught. Valuable as this may be as a proximate generalization, it seems to carry with it the logical deduction that the normal consciousness is hedonically indifferent: in other words, that pleasure and pain are derivative and not original qualities of consciousness. I think, on the contrary, that the weight of psychological authority will always favor the view that consciousness is originally hedonic, a position which the author does not, in fact, distinguish from sensationalism with which it is not identical. For it is possible to maintain that pleasure and pain are original *quales* of consciousness while rejecting the theory criticised by Mr. Marshall; namely, that pleasure and pain are psychic elements from which all other mental phenomena are derived. Sensationalism is not the only alternative here. Besides, I fail to see how Mr. Marshall's own theory can be grounded without recognizing the originality of the pleasure-pain *quale*. His formula seems to explain, not the rise of pleasue and pain, but simply their *variation*, while it tacitly assumes an original hedonic consciousness. If this assumption be admitted, then the last word has not been said about pleasure and pain, whereas, if it be denied, the conclusion is ineivtable that consciousness is not, in itself, pleasant or painful, a conclusion which I think sound psychological analysis will disprove, for it seems to me that introspection teaches that normal movements of consciousness tend to be pleasant. Genetically I think there is a balance in favor of the position taken by Baldwin that pleasure and pain arise in the first instance in connection with the first stimulations of the environment. This follows necessarily on the view that consciousness *per se* possesses the hedonic quality. Such a view does not, however, supersede Marshall's, but rather subsumes it under a more primary law, since if pleasure and pain are inseparable from consciousness, the law of its *variation* in consciousness will have its physical basis in the relation of nerve supply to the functional demands.

I find myself in accord with much that the author says about the emotions. But it seems to me that he had not all the possibilities in mind when he, as he in effect does, conceived Sensationalism and his own dual theory to be the only alternatives on the hedonic basis. A third alternative, to my mind more satisfactory than either, arises when we connect the hedonic quality of consciousness with its presentational elements and conceive the emotions to arise out of the synthesis. This enables us to explain the characteristic objectivity of the emotions as well as their complexity without breaking the unity of the mental life by a confusing dualism. I am not here criticising dualism *per se*, but the real dualities in this sphere are, I think, the original distinction between pleasure and pain and also that between pleasure-pain and the representational function of consciousness.

Personally, I confess to some dissatisfaction with Mr. Marshall's excellent treatment of the relation of æsthetics to pleasure and pain. It seems to put pleasure-getting too much in the immediate foreground. It is of course true that beauty gives immediate pleasure. But it is not so obvious that the getting or giving of this pleasure is to be taken as the aim of art or that it supplies its primary criterion. It might seem to be obviously so from the observer's standpoint. But it seems to me that the observer, if his conscious aim is to get pleasure from the contemplation of works of art, misses the true standpoint of art appreciation; whereas, if the point at issue is simply the fact that he experiences an immediate pleasure from the contemplation, it is open to us to seek the grounds of this pleasure, which will be found, I think, in the fact that it satisfies in some way the observer's ideal. This brings the observer's standpoint into line with that of the producer which is concededly the realization of his ideal. The immediate object of the artist thus stands as the ultimate object of the observer. Now it is obvious that if the artist consciously aims at pleasure rather than the ideal, he mistakes the artist's standpoint for that of the moralist or social philosopher. And when we consider the question of the ultimate reference of the artist's ideal, I think, we will find it not to be pleasure getting or giving, but some kind of *realization*. This leads me to think that pleasure supplies only a secondary aim in æsthetics and that its primary aim is to be sought in the ideal reference indicated above.

PRINCETON. A. T. ORMOND.

CHILD PSYCHOLOGY.

Evoluzione del Senso cromatico nella Infanzia. A. GARBINI. Estratto dall' Archivio per l'Anthropologia e l'Etnologia vol. XXIV, Fascicola 1° e 2°, 1894. Florence, 1894. Pp. 58.

This is a piece of work done at the laboratory of infantile psychophysics at Verona. After giving some account of the investigations of Vierordt, Uffelman, Preyer, Binet, and others, Dr. Garbini definitely sets himself to answer the following questions: (a) Whether the perception of light precedes that of colors, and when, approximately, each of these makes its appearance; (b) In what chronological order the principal colors are first perceived; (c) What would be the most logical method of cultivating the sense of sight.

About six hundred children were examined, and the results very clearly set forth, with the aid of numerous valuable tables. Only a brief outline can be given here:

One would naturally expect that the child's sensibility to light would be developed earlier than any sensibility to differences of color; since the latter requires the separate action of specific spectral rays, while the former is due to an action common to all those rays. And this Dr. Garbini finds to be the case. The new-born child apparently *feels* the light—experiences from it a sort of photodermal sensation which is general and not special in locality. Moreover, the infant during the first five days is distinctly averse to strong light falling upon his eyes; he is *photophobic* by reason of retinal hyperaesthesia; hence Dr. Garbini names this period *il periodo fotodisferico.* During the second period, (which extends from the 5th to the 30th day), the child passes from the condition just described, (*fotofobo*) to a state in which he finds diffused light agreeable, (becoming *fotofilo*), and also accomplishes his first simple perceptions of the *light* and *dark.* This period, therefore, is named *il periodo fotoestesico.* The third period extends from the 2nd to the 16th month, and is called the visual period, to mark the very important advance —made usually about the second month—from passive to active sight. The infant now for the first time directs his gaze, without turning his head, towards luminious objects not too far away, (28–35 day), and later learns to follow with his eyes an object moving slowly from its place (7th week). The fourth period, which begins with the 16th month, Dr. Garbini calls the chromatic period (*il periodo cromatico*), because here the differentiation and perception of colors begins. He employed two methods side by side in making his experiments: The first, (which he calls *metodo muto*),

consists in having the child select, from a pile of colored skeins, one to match the one that lies before him. This method, it will be observed, differs from that employed by Binet in that it requires the exercise of perception only, whereas Binet's method draws upon memory as well. The second, (*metodo verbale*), consists simply in getting the child to name the color of the skein presented to him. The result of these observations is to place the principal colors, in the chronological order of their first perception, as follows: Red, Green, Yellow, Orange, Blue, Violet. Here, it will be noticed, yellow is third in the order of perception, whereas Preyer places it first. Green, too, stands unexpectedly near the head of the list, since both Preyer and Binet place it last but one in their tables. In somewhat more detail the results are as follows: From the 16th to the 20th month the child begins to distinguish Red. From the 20th to the 24th month he learns to distinguish Red better, and to have some confused knowledge of Green. In the 3rd year Yellow is perceived with some uncertainty, and there begins some tentative perception of Orange. In the 4th year the child distinguishes Red quite readily, Green and Yellow not quite so well, and begins to differentiate Orange, Blue and Violet, provided they are tolerably fully saturated. In the 5th year he differentiates Red, Green and Yellow quite well, and Orange and Blue with some difficulty, confounding the latter very often with Violet. One may notice also at this time an advance in the child's ability to distinguish shades. In the 6th year Red, Green and Yellow are perfectly distinguished; Orange, Blue and Violet not so perfectly, but toleraby well; and shades come to be very readily recognized, though considerable difficulty is still encountered in distinguishing tones which lie next each other in the spectral series, especially if their saturation is not very marked. One must say, then, on the whole, that the chromatic sense is still imperfect, even at the close of the period of infancy (end of 6th year). Dr. Garbini finds 2 children in every 100 who at that age cannot name any color; and he finds only 35 in every 100 who can name all the colors readily.

The order in which the child learns to *name* the perceived colors, is, as might be expected, the same as the order in which the colors themselves are learned; but this name-series, though parallel with the perception-series, is not synchronous with it, but lags about a year behind; *i. e.*, the child learns to name any given color correctly about a year after he has learned to know the color itself. This is explained by the consideration that the act of connecting a color with its name is a higher and more complex mental act than that of

merely perceiving the color. Sex seems to have but little influence on the development of the color-sense. Boys he finds rather more advanced about the fourth year, girls in the fifth and sixth years. It is a somewhat remarkable circumstance that these investigations revealed not a single case of color-blindness, though nearly six hundred children from three to six years of age were examined. Those who are familiar with the interesting researches of Jeffers, Meyer and others on this point, will remember that these investigators found as high as 4% of the boys and from .6 to .24% of the girls, more or less color blind.

Dr. Garbini closes with a number of suggestions regarding the training of the sense of sight by means of appropriate exercises in chromatic and other visual discriminations; believing that such exercises, if based upon, and carried out in conformity with the natural order of development, might be of great service in accelerating the acquirement of visual facility and control. If the present writer is not mistaken, this very end is aimed at, and to some degree attained, in our kindergartens.

Transactions of the Illinois Society for Child Study. Vol. I, No. 2. Handbook for the use of Members and Round Tables; including a Plan of Organization and Syllabi for Work in Child Study. The Werner Co., Chicago and New York. May, 1895.

Those who are interested in the study of children will find many valuable suggestions in these Handbooks of the Illinois Society; while for that large class whose lack of interest arises simply from lack of information on the subject, one could scarcely recommend anything better than these little pamphlets, with their definite, brief syllabi and pointed questions. The present number contains the constitution and by-laws of the Illinois Society, an address by Col. Parker, setting forth its plan and purpose, directions for organizing and for undertaking the work of child-study, some twenty-one topical syllabi containing questions and suggestions for the guidance of investigators, and a bibliography of something like two hundred and fifty literary references on the subject.

Prof. Baldwin leads off with a syllabus on the study of the social sense in children, or the observation of cases of special friendship or 'chumming.' Dr. Dresslar gives an account of a plan for the investigation of the decline of habits, with a view to the eradication of evil habits in the child. Dr. Lukens and Col. Parker give directions for the study of child-language; and President Hall publishes again his syllabus on 'Fears in Childhood and Youth.' Drs. Scripture and

Gilbert show how to apply scientific methods to the study of childhood; and Dr. Bolton furnishes some valuable remarks on 'The Prerequisites of the Scientific Observation of Children.' We should free ourselves from our preconceived pedagogical and psychological theories. The student of childhood should be an observer and not a critic. Parents are apt to be poor observers because they 'have too much stock' in the pupil; teachers, because they have, as a rule, too little. Tests of the senses of children are suggested by Mr. O'Shea and Mr. Kinnaman, a schedule for the study of mental abnormalities by Dr. Adolf Meyer, and directions for anthropometric investigations by Dr. W. O. Krohn. One of the most valuable articles in the book is that by Dr. C. C. Van Liew, the Secretary of the Society, entitled 'The Study of the Child on Entering School.' The list of questions asked in this article regarding physical, mental, emotional and volitional development shows the writer to be a keen observer and a thoughtful Investigator. Every teacher would do well to study carefully this article and that by President Hall, entitled 'The Contents of Children's Minds on Entering School' (Ped. Sem., Vol. I, No. 2.) Among the remaining articles of the *Handbook* there are two topics of very great importance, viz., the child's imitation of his teacher (by Prof. Bryan), and the child's interests (by Prof. E. E. Brown). The teacher who has enough *interest in* the child to lead him to study carefully and systematically the prevailing *interests of* that child has taken a long step in the direction of preparation for successful teaching.

TORONTO UNIVERSITY. F. TRACY.

ANTHROPOMETRY.

Physical and Mental Deviations from the Normal among Children in Public, Elementary and other Schools. Anthropometric Work in Schools. Anthropometric Laboratory. Three reports of committees before the Oxford meeting of the British Association for the advancement of Science. London, John Murray, 1894. Pp. 434-453.

The British Association performs an important service in maintaining a large number of committees whose duty it is to investigate special subjects and report before the annual meeting. About fifty such reports were presented at Oxford, a majority of the committees being assisted by grants of money varying in amount from $25 to $500. The three reports recorded above are of psychological interest, and attention should be called to them, as the place of publication is such that they are liable to be overlooked.

The committee on deviation from the normal among children report on 50,000 children seen individually by Dr. Francis Warner, 1892–4. Of these 8,941 were found defective in some respect. The greater variability of the boys is not considered by the committee, but is of interest:—19% of the boys were defective and 16% of the girls. It is almost certain that desirable variations would also be found more common in the case of boys. The greater variability of the male is usual throughout the animal kingdom—the females of closely related species are sometimes almost indistinguishable. In so far as the same holds for man is a matter of considerable theoretical and practical importance. The committee give a table showing the nature and prevalence of the defects, the details of which are of interest. The statistics have, however, been obtained after a cursory examination by Dr. Warner, *e. g.*, those children wearing glasses were recorded but defects of accommodation were not determined, and his personal equation must largely influence the results. 'Nerve signs' is a relative term—other observers examining the same children might find these in twice as many or in half as many cases.

The report of the second committee inquires to what extent actual anthropometric measurements are now made in schools and suggests methods for carrying them out. Of 398 schools replying to the circular of the committee, 58 make measurements, but they are usually simply of weight and size. Only three schools test color-blindness, yet this test which could be made in a few seconds would give information of the greatest possible value to one boy in every twenty. We may hope that not only the measurements recommended by the committee, but also certain mental tests, as of memory and attention, may be gradually introduced into our schools.

At each meeting of the Association for the past seven years a temporary anthropometric laboratory has been fitted up, and a third committee gives the results obtained from 155 observers at the Nottingham meeting. The measurements are mostly physical and only become of interest when compared with corresponding results from other races or other classes of the community. It is worth noticing, however, that only one-third of the men had normal vision.

J. McK. C.

NEUROLOGY.

Zwei Fälle von Rindenläsion Ein Beitrag zur Localisation der Vorstellungen. C. WERNICKE. Arbeiten der psychiatrischen Klinik zu Breslau. Heft II. Leipzig, G. Thieme, 1895.

Grundriss der Psychiatrie. C. WERNICKE. Theil I. Psychophysiologische Einleitung. Leipzig, G. Thieme, 1894. Pp. 80.

Wernicke reports two cases of lesion of the cortex resembling each other closely. There was a defect in the middle third of both central gyri esp. of the ascending parietal, caused in one instance by a blow and fracture of the skull, in the other by the rupture of an artery. The chief clinical symptoms after the defect ceased to be irritative were: tactile paralysis of the right hand with relatively little disorder of the sensibility and of subtle motility. Both cases had a disorder of speech, not unlike that observed in general paralysis, persistent in the case with somewhat larger cortical lesion, transitory in the other. Wernicke classifies the disorder as transcortical motor aphasia. The most important feature in both patients is that they had difficulty in recognizing objects by palpation, although there was hardly any disorder of sensibility in the hand. Wernicke explains this tactile paralysis as a loss of memories (vorstellungen), and mentions the very frequent cases of peripheral nerve lesion that form a remarkable contrast to the two just reported by showing very extensive disorders of sensibility with very little impairment of recognizing objects by touch. In lesion of the peripheral nerves, the disorder of tactile interpretation seems to be almost proportionate to the disorder of the sense of position of the members used; consequently this ability of recognizing objects has been made dependent on the integrity of the sensation of position. This seems not to be correct. Wernicke reports a case of locomotor ataxia who has in his right hand marked dullness of the tactile sensibility and complete loss of the sensation of position. The localisation of such stimuli as are felt at all is preserved, and the patient recognizes with his hand many larger objects, but not small ones. In the left hand, the tactile sense is better preserved, there remains a trace of sensation of positions: almost every object of some size is recognized. These cases show plainly that the lack of tactile discrimination cannot be due to the slight defect of tactile sensibility in the two cases of brain lesion, but must depend on the cortical defect.

The anatomical substratum of the tactile conceptions is formed by groups of cells connected by association fibres; the same concrete

object, the same sensation elements, are excited in the same arrangement and sequence, as often as the tactile process is repeated. The ganglion cells of the cortex (which ones?) represent the sensation element; the association of the sensation-elements is brought about by the association fibres. The functional groups thus formed for the tactile preceptions must be located in the part of the cortex which was found destroyed in the two cases reported. Of course, the patients were able to recognize objects with their left hand because the corresponding cortex of the right hemisphere was intact; but cases of so-called asymbolia show that the perception of objects touched is completely lost, as soon as the same parts in both hemispheres are destroyed. Wernicke comments on this statement in a note as follows: "Without any prejudice the defect can be defined, that the conceptions are no longer produced by the process of touch, the cause of this would then be left to further investigation."

A further point of interest consists in the relation between loss of conception of movements and conceptions of writing. Both patients recovered the use of their hand and fingers for almost all single and combined movements, even where tactile guidance was excluded (movements of opposition of fingers). The great difficulty in writing disappeared together with the difficulty in finer manipulations generally. A special center for agraphia seems not to be probable.

Both cases showed plainly that the distribution of the paralysis and the disorder of sensibility shortly after the injury followed the subdivision given by the articulations, leaving the movements of shoulder and elbow perfectly intact. Pain and temperature sense remained unaffected; tactile sensibility was first diminished on the forearm and hand, but returned in a few days; but the sense of localisation remained defective in the hand and fingers.

These observations are of importance because Wernicke bases on them and on his theory of aphasia the psychophysiological introduction of his *Grundriss der Psychiatrie.* It is practically a brilliant attempt of translating the psychological language into the terminology used in dealing with diseases of the brain. Whether it will prove to be more remains to be demonstrated by the practical part of the work. However paradoxical and dogmatic many of the views are, they cannot help rousing thought in the direction of rational psychology. It is difficult to do the work justice in a short review; but the main points may be summed up as follows:

In his theory of aphasia, Wernicke recognizes a sensory projection field S and a motor projection field M. M is the place of

origin of that part of the central motor path which transmits the impulses for the articulation of speech and, at the same time M is the seat of the '*Sprachbewegungsvorstellungen*,' or 'memories of the movements of articulation.' The Sensory field S. is the central end of the auditory path and the seat of the memories of the '*Klangbilder der Worte*' or auditory word images. As such it has the function of *primary identification* of the sounds. The *secondary identification*, the connection of sound and idea, depends on the function of the transcortical association fibres and of the center of ideas, which, of course, is only a schematic term and is really distributed over parts of the cortex very distant from each other. The connection between the supposed center of ideas and the center of motor memories of speech would form the terminal path of the apparatus of secondary identification formation of answers and speech generally. All strange utterances of the insane are symptoms of disturbed secondary identification and Wernicke feels justified in generalizing this and in saying that mental diseases are disorders of secondary identification and have their seat in the transcortical or association tracts without affecting the projection systems except where focal symptoms are sometimes present, as in general paralysis.

The regularity of the association is due to the principle of '*Bahnung*.' The only objective signs of the character of mental processes are the movements which W. subdivides into movements of expression, of reaction and of imitation, three groups that overlap considerably. The following classification would give all the possibilities of disorders of secondary identification :

Psychosensory:	Psychomotor:	Intrapsychical:
Anæsthesia	Akinesis	Afunction
Hyperæsthesia	Hyperkinesis	Hyperfunction
Paræsthesia	Parakinesis	Parafunction.

While W. limits the faculty of memory unduly to the nerve cell bodies (for which view he gives only evidence of probability (p. 22) and against which the theory of the neuron seems to speak), he admits that no special memory-cells are needed. The memory image is nothing but an acquired association of perceiving elements of the central projection field. The difference between memories and after-images of the retina is explained by the fact that the cerebral cortex has association fibres, whereas the retina has none. One of the most important associations of the perceiving elements of vision is that with the memories of the movements of the eye.

Associations of memories of sensory impressions form the 'concrete idea' with its essential and unessential qualities. Their sum is our consciousness of the outside world.

Most associations depend on simultaneity of impressions; another kind depends on the *sequence* of impressions and in a similar way on the principle of the *ausgeschliffenen Bahnen* (traces worn). It is this latter form of associations on which reasoning and our knowledge of the order and causation of things depend. 'The insisting on a knowledge of causes (*Causalitätsbedürfniss*) is an inborn defect or quality of our brain.' The anatomical explanation would be as follows : Pathways would connect the places where the memories of each sense are localised. The regularity in the activity of certain association tracts will correspond to the regularity of things happening outside. The associations are not, however, a picture of the connection of the things themselves, but only the traces left by them in the sensory fields ; hence their subjective character. The possibilities of connections seem so numerous that Wernicke resorts after all to the hypothesis that the psychological unit of a memory also corresponds to an anatomical unit. They might be, for instance, the cells of certain cortical "layers which receive fibre processes from the association cells, but at the same time, would be connected with the perception or projection cells." Thus we meet after all the 'memory-cells,' although they were said to be unnecessary.

Lecture 5 treats the consciousness of our body, of the 'primary ego,' as an acquired function of the central projection fields. After a very interesting description of his views on the development and importance of 'local signs' and organic feelings as part of the organic consciousness, Wernicke enters again the field of speculation with regard to the central projection fields. *Organic consciousness* is represented by the perception cells, a primary station of the cortex, which must be passed before the sensory impression reaches the *consciousness of the external world.* "The stratification of the ganglion cells of the cerebral cortex favors such a theory according to which the layer or layers next to the marrow would represent the '*Bewusstsein der Körperlichkeit*' or organic consciousness." This statement almost reminds one of the naive idea of Baillarger, that the stratification of the cortex has a great and by no means accidental resemblance with a voltaic pile.

Lecture 6 treats the importance of the movements and their relation to the organic sensations. The 7th lecture on the consciousness of the personality ends with the statement that self consciousness is really an illusion inasmuch as the mental personality does not perceive itself, but that personality that existed a few moments, hours, days, or even years previous. The last lecture is a clever application of the results obtained to a picture of mental life, lead-

ing over to the special field of psychiatry. The problem of attention is considered with special reference to the power of application of attention, called '*Merkfächigkeit*' by Wernicke, both with regard to the acquirement of new memories and to the remembrance of old ones. The emotion and the relative import of conceptions are studied last and lead over to a consideration of abnormal emotions and delusions, etc.

The little work of only 80 pages contains so may interesting remarks that it cannot be done justice in such a short recapitulation. It shows in a concise and clear manner the trend of thought prevalent among alienists and physicians and is based to a great extent on the previous works of Wernicke. At the same time it shows the wide gaps between our actual knowledge of the functional anatomy of the nervous system and psychiatry and rational psychology all the more as it tries to build numerous bridges with a certain amount of optimism.

HOSPITAL, ILLS. A. MAYER.

VISION.

Ueber die Erkennbarkeit des Sehpurpurs von Abramis Brama mit Hülfe des Augenspiegels. G. ABELSDORFF. Stzber. der Akad. d. Wiss. zu Berlin, 4 Apr. 1895.

Zur Darstellung des Sehpurpurs. O. KÜHNE. Ztsch. of Biol. XXXII. 21–28.

When Boll first discovered the visual purple, he was of the opinion that part of the red color visible when the eye is looked at through the ophthalmoscope is due to this substance. Becker and Coccius, among others, showed that this is not the case, but that the visual purple in the living eye is completely masked by the thick layer of blood-vessels behind the rods and cones. After it was known that in ultraviolet light the retina fluoresces a light blue when the visual purple is present, and a light green after it is bleached out, it was thought that in the case of an individual whose lens had been extracted, the condition of the visual purple could be detected by this mark; but the fluorescence turned out to be not sufficiently strong, and nothing was accomplished by this means. But Dr. Abelsdorff, working in König's laboratory, has at last been able to watch the gradual fading out of this substance in the living eye. Besides the more common choroidal tapetum, some fishes are provided with a retinal tapetum, formed by a layer of highly reflecting substance in the epithelium cells. Against this white background the visual purple can be plainly seen, through the ophthalmoscope, and

it can be watched at one's leisure as it gradually gives place to the visual yellow, and then entirely disappears. For the details of the proceeding, the original paper must be consulted.

Kühne has at last been able to perfect a method by which the visual purple can be extracted free from every trace of haemoglobin, and hence particularly well adapted to the critical experiments which now have to be made with it. He also succeeds, by means of purifying it with magnesium sulphate and drying it in vacuo over sulphuric acid, in obtaining a dry powder which can be permanently kept, and used at any time, and with any color-depth, in examining the properties of this substance. This important improvement in the method of handling and preserving the visual purple ought to lead, in the light of König's recent researches upon it, to a renewed interest in its study.

BALTIMORE. C. LADD FRANKLIN.

Ueber den Nachweis von Contrasterscheinungen im Gebiete der Raumempfindungen des Auges. J. LOEB. Pflüger's Archiv. f. d. gesammte Physiologie, LX, 509–518. 1895.

That a spatial sensation becomes modified, if a second spatial sensation is simultaneously produced and attended to, is seen in many well-known optical illusions. Whether this influence ever takes the form of a contrast is, says Loeb, still an undetermined question. It is assumed by some for the directions of lines, but this view is supported only by the very illusions to whose explanation it is applied, and other explanations of these illusions are given.

A simple experiment, however, proves the existence of this spatial contrast. Let the experimenter, with head fixed, place on a horizontal table two lines or points in such position that they shall be equally distant (about 40 cm.) from the intersection of the median plane of the observer with the table, to the right of it, and parallel to it. Place now a third line or point by the side of the nearer one of the other two. The latter will no longer appear to be at the same distance as the farther one from the median plane, but if the third be placed nearer the median plane than the second the latter will appear to be displaced toward the right; if farther, toward the left. The same illusion appears in experimenting with distances away from the observer, as with distances to right or left. Attention to both the inducing and the influenced objects is necessary in order to produce the contrast effect; hence the effect is greater the nearer they are together. The average apparent displacement, measured on eight persons, is 3-6 mm. This spatial contrast explains the fact that

shorter distances of objects from one another appear relatively larger than longer distances; also the fact that two points appear farther apart if other points are placed between them.

No attempt is made by Loeb to explain the illusion which he describes. He merely establishes it as a fact, and calls it a case of contrast; but he does not tell how the contrast is to be accounted for.

Untersuchungen über Farbeninduktion. Kr. B. R. Aars. Kristiania, 1895. 15 pages. 3 tables.

When colored surfaces are placed near one another, each affects the impression received from the other, in some cases by contrast-induction, in others by color mixture, called by Aars syncrasy-induction. To investigate these effects, Aars used discs covered each with sectors of two colors, those of the inducing color having an angle of 45°, of the induced color 15°. The effect of eleven inducing on seven induced colors was noted. The distance of the observer from the discs was for the most part seven meters. His results are: (1) Contrast-induction takes place under wider differences of brightness between the two colors than is usually assumed, especially in orange and in green. (2) Syncrasy diminishes and contrast-induction increases when the inducing color lies (in its position in the spectrum) in the immediate neighborhood either of the induced color or of its complementary color. Orange and green, however, offer exceptions. Syncrasy takes place instead of contrast when orange is combined with colors lying near its complement (the blue colors), and when green is combined with its own neighboring colors to the right (likewise the blue colors). This is because the blue colors possess an unusually large coloring power. Yellow shows varying results, easily losing its character as a true color, and leaving free play to the rival influences. White sectors show syncrasy as well as contrast-induction.

These results Aars presents graphically in curves, showing the results of the combination of each induced with each inducing color, and comparing them with the results obtained by the use of rotating discs.

Ueber das sogenannte Purkinje'sche Phänomen. E. Hering. Pflüger's Archiv. f. d. gesammte Physiologie, LX, 519–542. 1895.

If in a light of moderate brightness two colors are chosen of apparently the same intensity and saturation, and the illumination is then diminished, the blues and greens will appear brighter than the

reds or yellows. This is Purkinje's phenomenon, which heretofore has received no satisfactory explanation. Hering investigates and explains it in his characteristically thorough manner. For its production he arranges color glasses before two holes in a door, communicating between two rooms, both of which can be separately darkened, in one of which, whose walls and door are made entirely white, sits the observer, and in the other of which is a white screen which reflects light through the holes. This arrangement permits a darkening, either of the colored surfaces alone, or of the field about them alone, or of both together. He demonstrates the following facts; the explanation of the phenomenon is given most fully under number 4.—1. The simultaneous and equal diminution of the light-intensity of two colors is not sufficient to bring about the Purkinje phenomenon. This can be shown by diminishing the illumination of the screen-room, leaving that of the observation-room unchanged. The blue or green does not then become brighter than the red. Yet Helmholtz had regarded the change in intensity of the compared colors as the essential condition of the phenomenon. This experiment shows that he was mistaken. When blue and red papers are compared, and the illumination of the whole observation-room is gradually diminished, the blue does appear brighter than the red; and in the end the former may become colorless white, the latter colorless dark-gray. The difference between the two experiments is, that in the former the eyes retain their general adaptation for brightness, in the latter they become gradually adapted to darkness. The change in Stimmung, or sensitiveness to light, is then the essential condition of the phenomenon; and this fact is clearly established by the next experiment. 2. The mere change in sensitiveness (Stimmung) of the portions of the visual apparatus excited by the colors is sufficient to bring about the Purkinje phenomenon, the light-intensity of the two colors remaining unchanged. To show this, darken the screen-room until the colors are but just recognizable; then, leaving their intensity (which remains still apparently about equal) unchanged, suddenly darken the observation-room. The Purkinje phenomenon appears. The illumination of the retinal portions affected by the colored fields remains unchanged, but their Stimmung is altered by the darkening of the surrounding field. The sudden change in Stimmung, which the entire visual apparatus undergoes in this rapid darkening, Hering calls the instantaneous-adaptation for darkness, in distinction from the lasting-adaptation which results from remaining a considerable time in darkness. By covering-over one eye during the night after considerable sleep, and

keeping it closely covered until the experiment is tried, it is possible to compare the appearance of the phenomenon to an eye with instantaneous-adaptation, and to one with lasting-adaptation for darkness. In both cases the phenomenon is more striking when the colors are observed in indirect vision. 3. The phenomenon appears when there is a simultaneous change in the Stimmung of the eye and in the light-intensity of the colors. This can be shown by darkening both rooms, or by the ordinary observation of the phenomenon with colored papers. 4. The Purkinje phenomenon is characterized as much by the change in saturation of the colors, as by the change in their relative brightness. If we take two colored papers, spectral red and spectral blue, which in daylight appear of nearly equal brightness, and then gradually darken the room, both colors become less saturated, and the change in saturation of the red is entirely different from that of the blue: the latter becomes constantly whiter and finally a colorless light-gray, the former darker until it is colorless dark-gray. This gradual disappearance of the colored components as against the colorless, and thus the change in saturation of the colors, is a *conditio sine qua non* of the Purkinje phenomenon. It has been heretofore overlooked, yet it is responsible for the change in brightness of the colors, and it depends itself upon the difference in the admixture of white with the original spectral colors. If a blue and a red are chosen which have an equal admixture of white, the red in good illumination will appear brighter than the blue, which difference will gradually become less if the illumination is diminished. A blue which appears in daylight of the same brightness as a red, has really a larger admixture of white: for, as Hering has previously shown, a red or yellow component has a relatively brightening effect, a green or blue a relatively darkening effect, on the total sensation. In the ordinary demonstration of the phenomenon, we choose a red with a small, a blue with a larger admixture of white. In ordinary light they appear of equal brightness; when the illumination is lessened, the colored components produce less and less effect, the colorless become more apparent and their difference is emphasized by the adaptation of the eye for darkness. Hence the brightening of the blue and the relative darkening of the red. 5. The Purkinje phenomenon can be produced by a mere change of the illuminated portions of the retina. The sensitiveness to white increases, that for colors diminishes, from the centre of the visual field toward the periphery. This is especially true when the illumination is slight, and the eye is thus instantaneously or lastingly adapted for darkness. If then we darken considerably first the screen-room, and

then the observation-room, a red and green may still appear saturated and of equal brightness by direct vision; but to indirect vision the green will appear brighter, because the different admixtures of white in the two colors will become more perceptible. 6. The Purkinje phenomenon is essentially unchanged, when homogeneous spectral light is used, in place of colored glasses or papers. 7. König has made extended measurements, in which, having made two different spectral colors of the same apparent brightness, he repeated the operation for different degress of light-intensity of one of the lights. He found that intensities of the second light were necessary, in order to make it appear equally bright, which showed a constantly different relation to the intensity of the first light, varying with the degree of intensity of the latter. These measurements are unreliable, because the apparent intensity of the lights he used depended not only on their 'absolute intensity,' but also to a large extent on the momentary and changing adaptation of the eye ; and he took no account of the latter factor.

BROWN UNIVERSITY. E. B. DELABARRE.

Die Spontane Umwandlung der Nachbilder der Sonne in reguläre Sechsecke oder Acktecke. G. WAGNER. Zeitsch f. Psy. Band IX, Heft I. Pp. 17–22.

Subjective Visual Sensations. W. R. GOWERS. The Bowman Lecture, delivered before the Opthalmological Society on June 14, 1895. The Lancet, June 22 and 29. Nature, July 4 (condensed).

Dr. Wagner has observed that the circular after-image of the sun may in his case spontaneously change to a hexagon or an octagon. The change usually occurs in the negative phase of the image, but after it has taken place it lasts until the image disappears. He sees the phenomenon best when he gets by accident an image of the setting sun and on the periphery of the retina. He says the beginner must practice regularly for fourteen days before he can expect to see the images, but does not make it clear whether or not his observation has been confirmed by others. The 'beginner' should, however, be very careful in undertaking to observe after-images of the sun, or he will suffer the experience so graphically described by Newton. The hexagons observed by Dr. Wagner seem related to that seen by Purkinje (*Physiologie der Sinne*) on revolving wheels—which he could only see with one of his eyes and which does not seem to have been subsequently confirmed—and the hexagonal subdivision of the field of vision described by Dr. König (Gräfe's Archiv, XXX). The hexagons would seem to be due to some structural

peculiarity of the retina, but no anatomical basis has been observed, and it may be necessary to seek for it in the brain rather than in the retina.

Dr. Gowers in an interesting lecture describes the subjective visual sensations preceding epileptic fits and megraine or 'sick-headache.' In the case of epilepsy the images may be 'stars,' flashes or luminous spheres, or they may be hullucinations. The image often crosses the field of vision and is followed by movements of the head and eyes. A curious fact is that bright reds and greens may appear in the periphery of the retina beyond the field usually assigned to color-vision—a fact the present writer has observed in the case of after-images. Dr. Gowers exhibited and deposited with the society sets of drawings showing images which occurred as precursory symptoms of sick headaches. These are zig-zag lines resembling the outlines used in fortifications. The angled and zig-zag lines seem related to the hexagons noticed above. Dr. Gowers thinks the phenomena are due to discharge in cerebral centers, and the present writer believes that this is correct and that it is not reasonable to attribute the complex phenomena of vision chiefly to the retina. J. McK. C.

ETHICAL.

The Rise and Development of the Moral Feelings. A. A. Tokarskiy. Voprosi filosofi, VI, 1, Jan., 1895.

We receive from the external world feelings and groups of feelings (or perceptions) which function in memory as ideas and concepts. Each feeling and perception is accompanied by a felt tone, which is either pleasurable or painful. These qualities of pleasure or pain awaken corresponding ideas and concepts, only weaker in degree; but unlike the original feelings, which proceed by reaction of the organism upon external stimulation, the corresponding ideas and concepts are dependent exclusively upon the nature and social position of the percipient. Consequently as from the ideas one cannot always infer the corresponding feeling, just so the same feeling hardly ever accompanies the same idea. This general psychological observation may apply to those feelings called moral.

Moreover since every feeling has in consciousness a corresponding idea we find at the same time growing up through a process of abstraction a few universal principles to which each class of feelings is invariably referred. That is, as we know the percepts and ideas with which moral feelings are united in us, just so we are able to

find the universal—the known abstract formula, which will show to us in all its generalizations, the object of moral feelings, and which at the same time will give them their most general characterization. All experiences divide into two groups: (a) Those that suggest the desire to govern; (b) those that do not.

Those experiences that suggest the desire to govern are such as satisfy the needs of body or mind—personal needs, satisfaction of which can give only pleasure. As such they are quite relative to the individual, and are in consequence termed Egoistic feelings.

On the other hand, those experiences that cannot suggest the desire to govern, do not remain indifferent to us; but according as each stimulation by our essential nature is pleasurable or painful they are in every case distinguishable.

It is in this way we find moral feelings proceed under the two general categories (a) of pleasure, as determined by what is good for the organism; (b) of pain, as determined by what is bad for the organism. Along with the sense of pleasure, as determined by what is good for the organism, follows the desire that the object of that pleasure should continue to exist; and in turn the feeling that threatens dissolution of pleasure gives uneasiness or pain. That is, pain at the loss of a certain pleasurable feeling that has been good for us in the past, gives, at the same time, increased feeling for life, which in its very tone as pain (or pleasure) is the beginning of the moral sense.

The Egoistic and Altruistic moral Feelings do not then find their respective meanings in a qualitative difference, but since both rise in the desire for increased life, they rather differ in the nature of their object. The Egoistic terminate in the desire to govern, and are thus individualistic. The Altruistic find meaning in reference to an object world as such; while from the standpoint of the Egoistic feelings it is possible to show how man might have developed moral feeling in perfect isolation, yet, as a matter of fact, man has from the first displayed social impulses which give moral feelings their other aspect as altruistic.

Altruistic feelings are exhibited under the rubric of Sympathy. Those feelings that suggest or interpret Sympathy are the most intensive states such as the mere affects of joy or sorrow, so far as these states are communicable.

Sympathy may be generically defined as the general capacity to reproduce by oneself feelings experienced by another. Under this general rubric two specific forms must be distinguished.

a. Organic sympathy arises from purely physiological peculiari-

ties of similar organisms and as such is a phenomenon to be experienced only in a community of like needs and interests.

b. Condolence or Co-suffering, as a specific form of Sympathy, is the capacity of reproducing in oneself, the pain experienced by another of like constitution.

From the above specifications, it may be inferred, moral feelings, however egoistical in origin, are only experienced in full (as worth) when man contemplates the object—nature and man, which by its universality and intensity drives out the lower Egoistic moral feelings. The subject is further developed in its social bearings.[1]

PRINCETON. LESTER JONES.

Werththeorie und Ethik. CHR. EHRENFELS. Vierteljahrschrift für wissenschaftliche Philosophie, 1893, I, 76; II, 200; III, 321; IV, 413; 1894, I, 77.

In these five articles the author treats of his subject under three topics: an analysis of the general idea of value, the theory of the variation of value, and its special relations in the sphere of ethics. There is an introduction with criticism of the labors of Menger and Wieser in this field. Ehrenfels regards value as proportional to utility, and subject to the law concerning the final degree of utility, which is determined by the supply and demand ratio. Utility is the capability of satisfying need, either appeasing desire or relieving discomfort and pain; this is not in relation to the passive states of pleasure and pain, but rather to the active wishes, the striving and willing. Value, therefore, is in proportion to the intensity of desire. It may refer to the worth of an object in and for itself, or merely for what the object is able to effect. Utility may be regarded in a special or general sense; as special (*Nutzen*), it is that which produces value which can be interpreted in terms of happiness, as general (*Frommen*), it is that which creates value of any kind whatsoever. Concerning variation in value, Ehrenfels emphasizes its psychological basis in that continuous changes in desire and general disposition produce variations in our estimation of objects which are capable of satisfying our varying needs. Moreover, different persons possess different aptitudes, some of which are original, others acquired in process of development of the organism, others again are occasioned through individual propensity to variation. Natural desires are modified in various ways through psychological laws, such as that of habit, of disuse, of association, or of the transfer from feeling to its cause, thus awakening the potential feeling

[1] From the manuscript translation of A. W. Herdler.

through ideation, or through representation of the objects which are calculated to arouse the feeling. Moreover, the influence of mind upon mind is a force which modifies one's idea of the pleasurable and the painful. This force may act through compulsion, example, or suggestion. The latter may be either normal or hypnotic. Value will vary with the desires thus modified.

Again, an object may be esteemed in one of the three senses: as means to an end, as an end in itself, or as the secondary consequences of the end. Variation in value may arise, either in the transfer of esteem from the end to the means, regarding the means as an end in itself, as the miser and his gold; or in the transfer of esteem from the end to the consequences of the end, as in the case of regard for food for its nourishment, rather than mere appeasing of hunger. Value thus derived through consideration of the more remote consequences attending the use of certain objects, becomes a factor in the development of the organism and the race. Those races have survived who have esteemed the various objects whose use has made for the preservation of life and increase of strength and power in the struggle for existence.

Each organism, moreover, has its own peculiar and limited sum of vital energy, and capacity for assimilation and adaptation. Desire will vary with the plus or minus of this vital energy, and this in turn will cause corresponding variations in value. The excess or deficiency in vital energy gives rise to four types of activity, resulting in mere preservation, in development, in arrested growth, and in degeneration.

Here follows a presentation of the theory of value in its ethical bearings. He draws a distinction between social and individual ethics. Ethical value is estimated in reference to the corresponding psychological state of elevation or depression which accompanies man's views of life's problem and the mysteries of existence. The feeling of elevation is mystical good. Whatever promotes it is good, and of value accordingly; whatever does not is evil. The psychical accompaniments, experiences, duties, etc., go to make up the ethical sanction. The highest ethical sentiment is love; and yet benevolence and sympathy, and all altruistic virtues must be coördinated with the purely individual virtues, as integrity, thrift, loyalty, etc. As to the worth-judgments, there are two types, the naïve and the sentimental, which are illustrated in ancient and Christian philosophy respectively. There is a like derivation of ethical value of an object considered in and for itself, from the utility of the object considered as means to end. Ethical worth is thus founded upon a

utilitarian basis. There are three categories of ethical value determined by the presence or absence of the utility factor, as follows:

1. Objects, or conduct still regarded as having ethical value which once possessed utility, but in course of development their utility function has disappeared. This gives rise to the two types, of arrested development and degeneration.

2. The normal ethical valuation of objects, or conduct whose utility is still existent and apparent.

3. Where the end to be realized is as yet ideal and not actual.

Ehrenfels suggests the question whether the law of the final degree of utility applies to the sphere of ethics. He calls attention to the fact that certain actions, as self-support, care of the young, while useful can not be considered as having ethical value, inasmuch as the predisposition to such actions is instinctive, and therefore always present in sufficient quantity and degree to further the preservation and welfare of the race. There is a necessity here of understanding the law of the increase or decrease of ethical supply. The tendency of ethical disposition is to manifest itself where it is most needed, and therefore most highly valued. There is a progress, moreover, in ethical estimation, in the formation of new ethical characteristics and feelings in man's aspirations after inner harmony with his social environment, also through man's progress in the better understanding of his social environment, and the conditions of its welfare, together with the continual widening of his ethical sphere of interests and activities. As to the ethical ideal, the principle of greatest good of greatest number must be supplemented by the principle of greatest progress. And yet it must be observed also that the greatest success and satisfaction follows one who is engrossed in the activity attending the pursuit, while the importance of the end is lost sight of for the present at least. In the striving itself there is real moral worth. From the standpoint of the ethical sanction, that condition or activity has greatest moral value which brings one into complete harmony with his environment, both present and future. In the appendix Ehrenfels gives an extended criticism of Brentano's theory of a good in itself as the basis of universal moral law. These articles abound in very careful analysis, and are rich in illustration and analogy. They furnish a utilitarian basis for morality, and are open to the general criticism of utilitarian ethic. It seems to me, also, that the idea of value cannot be preserved in all its manifold significance in passing from the economic to the ethical sphere. The analogy is suggestive, but there is danger of pressing it too far.

Zur Frage über die Freiheit des Willens. M. SWEREFF. Vierteljahrsschrift für wissenschaftliche Philosophie. 1893, IV, 476; 1894, I, 98.

The subject is treated in the first part in a critical manner, and in the second the author presents his constructive position. He finds a fundamental error in the traditional statement of the problem which recognizes an antithesis between freedom and causality. Free-will must then be considered as wholly uncaused and therefore essentially lawless. The result is that human activity is reduced, as Paulsen puts it to a 'series of disconnected and undesigned accidents.' Causation and freedom must therefore be considered, as the inner and outer aspects of one and the same phenomenon, and not necessarily in conflict. They can agree if considered as concepts of different categories, such as the categories of sound, and color. The theory of indifferent choice does not remove the difficulty. Choice, if it means anything, signifies a choosing for some reason, or some ground. An unmotived choice is the same as the lawlessness above mentioned. Again, we do not avoid the difficulty in the theory of self-determination. This does not clear, only obscures the question. The old problem still emerges,—is the self-determination through motive or not? The free-will dilemma still confronts us. Another theory that the will is an originally creative act, only suggests the some old query,—is the creative act according to some recognized end or not? Is there a reason for the act in question, or not? The same difficulty lies at the bottom of all these theories.

As to Swereff's constructive position, he holds that the question of freedom of the will concerns only deliberative actions. Where distinctly recognized alternatives are before us; all automatic or ideo-motor actions do not raise the question of free-will at all. He is free who chooses according to his reason. Reason regarded as motive to action has the power of silencing and ruling all other motives. This power varies in man according to birth, education, etc. It varies also in the individual at different times according to presence or absence of conflicting motives. Here arise also moral considerations. Every one possesses ideas of a distinction between good and evil as immediate deliverances of the judgment. Here it must be observed that law and motived regularity in the sphere of understanding is quite different from the same in the sphere of impulse. How then explain choice, and responsibility with this law of mental activity, namely the compulsion of reason? Responsibility must be regarded as incompatible only with a law of external necessity. Reason is the expression of the Ego and reason founded upon

law means that the law is itself the expression of the Ego. Determined by Reason means in the deepest sense self-determination. Responsibility arises from fact that conduct must emanate from *me*. Responsibility is for what I essentially am.

An objection is considered, namely, that reason in the last analysis is dependent upon the will, the fixation of attention, etc. This is answered when we consider the true concept of a unity of consciousness, in which feeling, reason, will all function. While all operating together, the predominating factor gives character to the resulting activity as will, reason, or feeling respectively. A second objection is that which is suggested by the statistical study of social phenomena, as observed in the labors of Süssmilch, Quetelet, and others. Here it seems as though regularity emerged in the midst of confused and widely different phenomena. Regularity in the aggregate is the law. There are laws of disorder as well as order. This is not incompatible, however, with the fact that the laws of reason should find objective manifestation in the activities and affairs of man. Swereff's discussion of this old problem is of value chiefly because of his psychological insight, and the fact that he attempts to solve the difficulties of the question by means of clear psychological definitions and distinctions.

Einiges zur Grundlegung der Sittenlehre. J. PETZOLDT. Vierteljahrsschrift für wissenshaftliche Philosophie. 1893, II, 145; 1894, I, 32; II, 196.

These articles are based upon a criticism of Franz Staudinger's '*Die Gesetze der Freiheit.*' Staudinger's theory of subjective contradiction, as the beginning of all volition, may be illustrated as follows: In hunger the impulse to action in order to satisfy the craving of appetite, arises from the mental representation of a contrast between some former state where food was at hand, and the present state where it is not. This creates a subjective tension towards some change which is the potential of the act of willing and capable of calling it forth. Some such experience characterizes the antecedents of every act of volition. Petzoldt modifies this theory substantially; he suggests that the subjective contradiction starts a series of activities whose end is stability. The mind is satisfied with that stability alone which can be expressed either in general concepts or in laws of nature. The end of science is to discover such concepts and laws, that in the repetition of any series eventuating in them, we would not wish any change whatsoever, but are satisfied to rest therein. The germs of this theory he finds in the theory of the vital

series elaborated by Richard Avenarius in his *Kritik der reinen Erfahrung* 1888–90, and in his *Der menschliche Weltbegriff* 1891. The theory of Avenarius concerned only the central nervous system, and was physiological in character. The law of tendency towards stability concerns physical, and psychical phenomena also. It is, moreover, supplemented in this particular by the law of '*Parallelismus*,' which is the law of the correlation of all modifications of the central nervous system with modifications of a psychical nature. Here follows a criticism of Wundt's '*Princip des Wachsthums der geistigen Energie*,' and of Wundt's '*Begriff der geistigen Causalität*.'

The moral ideal finally is that condition of psychical stability in which all aims harmonize. The law of stability as presented by Petzoldt, may be illustrated by the case of a woman and child found by a passer by perishing with cold. He seeks to rescue them, because he recognizes a condition of instability. His mind is ill at ease, restless under such a condition. He seeks to restore the normal and stable condition, at same time bringing his own mind into a like position of satisfaction and therefore stability. In all this, and in any voluntary activity whatsoever, Petzoldt accounts for all mental phenomena, simply by the three laws, of stability, conservation of energy, and the correlation of physiological and psychical phenomena. His theory therefore is purely phenomenal, and disclaims any dependence whatsoever upon metaphysics and metaphysical world-theories.

PRINCETON. JOHN GRIER HIBBEN.

PATHOLOGY.

Demon possession and allied Themes, being an inductive study of Phenomena of our own Times. JOHN L. NEVIUS, D.D., with an introduction by REV. F. F. ELLINWOOD, D.D. Fleming H. Revell Company, Chicago: New York: Toronto. Small 8°. Pp. x, 482. $1.50. [1894.]

This interesting contribution to mental pathology would probably fifteen years ago have gained for its author a reputation for nothing but mendacity or childish credulity in scientific circles; but now, thanks to the 'apperceiving mass' which recent investigations into trance-conditions have prepared, probably few readers of this journal will be seriously tempted to doubt its being a trustworthy report of facts. Dr. Nevius, for forty years a missionary in China, who died in 1893, is described by Drs. Ellinwood and Rankin as a man of rare learning, versatility and integrity. From the beginning

of his sojourn in China his attention was attracted to the popular belief in demons and spirits. He found before long that the native converts very commonly believed in demoniacal possession and in the power of Christian rites and invocations to exorcise the spirit. In 1878 he met with his first case, that of a non-Christian native named Kwo who, having bought a picture of the goddess Wang, had been visited by a demon-counterfeit of the goddess in a dream who told him she had taken up her abode in his house. Various neurotic conditions and disorderly impulses had followed, ending in an attack of frenzy during which, the man being unconscious, the demon spoke through his lips, demanding incense, worship, etc. As usual, the demands were met by the family, and the pacified demon thereafter made periodical visitations, throwing the man into unconsciousness and speaking through his organism, healing the diseases of visitors, and giving practical advice. On Dr. Nevius assuring Kwo that conversion to Christianity would rid him of the encumbrance, he became baptized, the trance-state only recurring once afterwards and the demon bidding a formal farewell on that occasion. Fourteen years have passed without relapse. Kwo has had persecutions and trials but no return of his malady, and neither he nor his neighbors think of doubting that he was rescued from the dominion of an evil spirit through faith and trust in Christ."

This case can serve as a type. Dr. Nevius has personally observed several others, and collected a large amount of information on the subject from other missionaries and from native Christians. The possessed persons are unconscious during the attacks, which have often, though not always, a convulsive character. The possessing spirit usually names itself, often as a deity, sometimes as a departed human being, and demeans itself accordingly. Sometimes it makes a formal treaty to behave well, on condition of certain favors being granted it. Sometimes it is driven out by threats or needle-pricks, etc. Christian rites seem to have extraordinary exorcising efficacy. Epidemics of possession, like those recorded in Savoy by Constans and by Chiap e Franzolini are not related by Dr. Nevius. The phenomena are among the most constant in history, and it is most extraordinary that 'Science' should ever have become blind to them. The form which they take in our community is the benign one of mediumship. Dr. Nevius is a believer in the reality of the alleged demons, and in the objectivity of their driving out by the name of Christ, etc. Such questions cannot be fairly discussed, however, till the phenomena have been more adequately studied. Dr. Nevius gives a large amount of collateral material and biblio-

graphical information; and we have to thank him and Dr. Rankin, the book's editor, for an extremely good contribution to a really important subject. W. J.

Studien zur Blinden-Psychologie. THEODOR HELLER. In. Diss., Leipzig. (Also in *Philos. Studien.*) 1895. Pp. 130.

Introduction. The difficulties in experimenting on the blind consist, says Heller, in the comparative rarity of persons who have lost all trace of color and light-sensations, and secondly in having usually to work upon psychologically unpractised persons who require training for the purposes of research. In the later case, the author recommends beginning with the investigation of the space-sense according to Weber's method, and also familiarizing the subjects with the usual psychological terminology.

I. *Touch in the blind.* The sense of touch is the only space-sense in the blind. In consequence of the limitation of the sense of hearing to the perception of intense qualities, a primarily spatial function cannot be ascribed to it. The blind use hand, tongue and feet in touching. Of these, the tongue possesses the greatest delicacy for spatial discernment. The hand adapts itself best to the exterior form of objects, the feet serve to measure step-movements and to touch objects lower in position. The system of touch-movements and the representation of spatial relations must not be considered as identical. Neither touch-movements nor the system of local signs can produce space, both factors always work together in the development of space representation, being related to each other as synthesis to analysis. The author distinguishes accordingly a synthetic and an analytic touch, but both species often mingle. Experiments proved, that two needle-points the distance between which about corresponded to the space-limen, were only felt apart under a certain normal pressure. With slight touches, or beyond this normal limen, the two impressions melted into one even below the pain-line. Finger-tips and joint-folds showed apparently the least normal intensity.[1] Investigations must always be pursued under the same conditions of temperature. Cold diminishes, warmth increases sensibility. These factors are of far greater importance in the blind than in the seeing. The '*Tastzuckungen*' of the blind already noticed by Tzermak are considered by the author as touch-movements which have become involuntary. As in the sense of sight the author distinguishes a 'direct' and 'indirect' touch, according as

[1] Concerning this see *von Frey's* recently published works. Berichte d. Math.-Phys. Classe d. Kön. Sachs. Gesellsch., d. Wiss. zu Leipzig. 1894–95.

the parts of the hand manifest touch-distinctions of the greatest or less accuracy. The '*unschliessendes Tasten*' (enclosing touch?), which may be performed by one hand or both and in which there is close union of outer and inner touch-sensations, serves for the reception of magnitudes of three dimensions. Synthetic touch transmits, however, only a scheme of objects, adequate representations can only be produced by aid of analytic touch. Instead of Loeb's term:—'Fuhlraum' of the hand, the author introduces the expression: 'Tastraum (touch-space), which he divides into major and minor touch-space (*weiterer und engerer Tastraum*), according to the arm-lengths necessary for its measurement in all directions. Minor touch-space is of the greatest importance in the production of precise space-representation in the blind; for within it are all conditions for synthetic as well as analytic touch. The measuring instrument is here formed by thumb and fore-finger often supplemented by the middle finger. The one hand fixes the object to be felt, the other performing the touch analysis, in which way the blind obtain the representation of the parallel source of two lines, of their convergence and divergence. The author further observed that the estimation of distances by the blind is often exact to a millimeter. The geometric-paptic illusions, observed in the blind by the author and explained by him as analogous to those of sight according to Wundt's theory, are highly interesting. The author disapproves the use of large models in instructing the blind, and recommends close adaptation of them to the relations of minor touch-space. Speaking of the development of touch-space in the blind, the author refutes the statement that the space-representation of the blind rests solely on touch-movements, and while instancing the very various individual development, once more emphasizes the fact that this spatial touch arises first in minor touch-space, representations thus obtained entering later into close relation with major touch-space.

II. *On the association of sensations of touch and hearing.* The statement that the sense of hearing is the real localizing sense of the blind, (so that, according to Preyer, Münsterberg and Kühnan, a person born blind with the surface of the body anaesthetic would acquire a complete space conception through impressions of hearing, and again that normal subjects born blind range their touch impressions in the hearing-space) is supported by the author only in so far as it is understood to apply at the most to the conception of the position of objects, or the direction of a sound-source, not to the representation of the objects themselves. Judging from experiments in which impressions of hearing were frequently erroneously local-

ized, (tone is in general oftener so than noises) the author designates the existence of hearing-space independent of touch or sight-representations as wholly illusory, proving further convincingly the associative connections of hearing-impressions with those of the sense of touch in the blind.

III. *On the so-called distance-sense* (*Fernsinn*) *of the blind.* These sensations are of the greatest importance for the protection and self-preservation of the blind. They do not depend on any special touch quality, nor are they primarily peculiar to any particular portion of the skin, only attaining their importance in the skin of the forehead which is highly sensitive to alterations of pressure. The medium of these sensations is the column of air between object and observer. Perceptions of hearing enter, however, as a very important factor into approach-sensations, but only bear the character of a signal-stimulus.

IV. *Surrogate-representations of the blind.* The author accepts this expression introduced by Hitschmann, designating a complex of representations which are present in the mind of the blind together with adequate representations and which are due; firstly, to a limited conception of space, and secondly to the lack of color and light-sensations. In accordance with these origins the author divides surrogate-sensations into a first and second category, examining in detail their importance in the intellectual life of the blind.

LEIPZIG. F. KIESOW.

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Thinking, Feeling, Doing. E. W. SCRIPTURE. Meadville, Pa., Flood & Vincent, Chautauqua Press, 1895. Pp. XII + 304.

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Die Schöpfung des Menschen und seine Ideale. W. Haacke. Jena, Costenoble, 1895. Pp. X + 487. M. 12.

NOTES.

The death is announced of the distinguished Swiss philosopher, Ch. Sacrétan.

Prof. W. R. Newbold, of the University of Pennsylvania, has become one of the associate editors of the *American Naturalist.*

Dr. J. H. Hyslop has been appointed Professor of Logic and Ethics in Columbia College, New York.

Prof. Stumpf's article in this Review for Jan., 1895, on *Helmholtz and the New Psychology*, has been reprinted in the *Archiv für die Geschichte der Philosophie.*

Dr. J. Allen Gilbert, of Yale, has been made Assistant Professor of Psychology at the University of Iowa. He will have charge of the laboratory work, for which new rooms and appropriations have recently been secured.

E. B. Titchener and J. E. Creighton have been made full professors in the Sage School of Philosophy in Cornell University.

Professor Thomas Henry Huxley died on June 30.

In the *Revue Philosophique* for June, 1895, is to be found a long and admirable account of the late Professor Charcot, by Professor Pierre Janet, under the title 'J. M. Charcot; son œuvre psychologique.'

We are informed that the earlier note in these pages (Vol. II, p. 328) to the effect that Professor Mach, formerly of Prague, had been called to a chair in Psychology in the University of Vienna, is incorrect. The chair which he assumes is that of 'History and Theory of the Inductive Sciences.' Further, Dr. Hillebrand becomes Asst. Professor of Experimental Psychology at Vienna.

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The Psychological Review.

THE CONFUSION OF FUNCTION AND CONTENT IN MENTAL ANALYSIS.[1]

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If we are not quite ready to say, with Heracleitus, that strife is the father of all things, at least it is a familiar truth that strife is the father of nearly all things that are good in scientific discovery. A truth we do not all so clearly see is that that unflagging controversy which is the life of a science requires a basis or background of comity and order to be truly productive. We must be agreed upon the ground of hostilities, upon the scene of action, upon the permitted weapons of warfare, before our campaigns can settle anything, or draw any definite frontier across the map of opinion to divide knowledge from error. And one thing more is needed if the best results are to issue from our disputation: the chances of war must be so narrowed and defined that we know without chance of mistake when we are defeated. The celebrated virtue of the British soldier is nothing better than a vice in the controversial engagements of science.

The capital importance of this last condition is far too little considered. The danger is a double one. The wasteful pertinacity that protracts discussion, when it ought to be applying its results, is not worse than those premature

[1] A paper read before the American Psychological Association in the session of 1893. I have not thought it needful to remove the traces it bears of the occasion of its delivery.

surrenders that have checked progress so often of late in such sciences as political economy, metaphysics and psychology. For such a surrender withdraws from the controversy an element of thought that should in its measure go to the making of the final result. The empiricists among you will not need to be reminded of the deplorable magnanimity of J. S. Mill, which prompted him to such gratuitous concessions of his father's principles, nor of how, the process once begun, his own principles melted away in the hands of his successors. If transcendentalism has almost fallen silent in Germany and empiricism forgotten at least its bolder tones in England, it can hardly be in *both* cases because unbiased reason has passed judgment.

Now, all these mischiefs of discussion are rife at the present moment in psychology. To contribute to the controversy is in many cases merely to contribute to the confusion; and, accordingly, it is well to turn to the more profitable business of defining that basis of agreement which is a chief means of making disagreement fruitful.

Among those unsettled matters which are in their turn unsettling to much besides, I take as one of the foremost the relation of content and function in the analysis of mental phenomena. There is a wide distinction here which in contemporary discussion is, as I think, too commonly obscured. A mental state is significant, of course, to the psychologist, not only for what it is, but for what it brings about; but it is widely assumed that what it brings about must in some sort be visibly reflected in what it is—in its intrinsic character. An idea has not only a particular 'content'; it has also a part to play in the mental life. It calls up other ideas; it influences the future course of thought or action. Now, the confusion to which I refer consists in supposing that mental causes, unlike physical, must themselves be an index, by the internal evidence they offer, of the train of consequences that they entail; that their function must be wholly determined by their content, and that accordingly their content is a sufficient key to their function. But this assumption, which is not self-evident, is further, I believe, entirely false.

Instances of what I mean abound; and I cannot bring the matter before you better than by drawing attention to a succession of cases in which the confusion and its attendant mischief are unmistakable.

The difficulties that figure in the time-worn dispute about the nature of general and abstract ideas are familiar. How can we employ our minds about a class of things if we are not in possession of an express class-idea, distinct from any idea whatever of concrete particulars? And yet how are we to frame a notion of a horse, for example, which shall represent neither a particular horse nor a collection of horses, which shall have color, but neither black, brown, nor any especial color; figure, but no one figure; size, but no exact size? How can we disengage those abstract elements common, as we say, to a class from the irrelevant particularities in which they are embodied and in which alone they exist. The difficulties of the problem are so obstinate that neither the so-called nominalists nor the so-called conceptualists, as a party, rest securely upon definite formulas, but covertly resorts at need to the language, if not the conceptions, of the other. The two opposing doctrines, in the strict extremity of their statement, seem the two horns of a hopeless dilemma, and the manifold compromises combine the difficulties of both.

Nor do these difficulties yield to the suggestion of Professor Huxley, Professor Sully and others, that the generic image may be a vague or blurred image, like a 'composite photograph.' The suggestion completely misses the point of the problem. A vague color or form, for instance, is a particular visual phenomenon and corresponds neither to all the colors or forms of its class (for it is single), nor to each of them (for they are mostly not vague); and it seems equally beside the mark to say that, although the conceptual image presents only some particular, yet it is coupled with an indefinable momentary 'sense of our meaning' which 'refers' the image to the whole class, and shows us how to 'interpret' and apply it. In order to *mean* a genus and not a particular, we must think of it, and how we can think of it is the very thing that gravels us. The verb 'to mean,' which

repeatedly of late has masked the begging of a psychological question, is in this case a somewhat thin disguise.

But there is a resort less unpromising. Are there not really detachable psychic elements identical in kind in our ideas of the different members of a class? To avoid irrelevant complications, consider at once a class of sensations or sensory perceptions; let us say the class of colors. One is prompted to answer distinctly, No: there is assuredly no identical element of 'color' discoverable alike in a shade of pure red and a shade of pure green, regarded as sensations. The two hues are 'similar' in so far as they are both colors, but they are in no respect 'the same;' they are not similar by having each a little of some one psychic material. But may not this answer be hasty? May there not be, invariably bound up with these colors in our perception and remembrance, some item of feeling incident to the physical process of vision? And might not this element remain the same, whatever color was before the eye, so that its presence would come to characterize the whole class of colors? Observe the issues here at stake. We speak of visual sensations and auditory sensations, for example, as of two classes self-evidently different—divided from each other by a broad disparity of sensational complexion. Yet there is an obscurity here. What is the tie of kinship between the color red and the color green which does not exist between the color red and the sound of a bell? We say the color and the sound are obviously less alike than the two colors; but wherein? Green and red are wholly and at all points distinct; how can red and the clang of the bell be more so? You see how we are touching the whole broad problem that Professor James and Professor Stumpf have discussed so suggestively—the problem of the analyzable or unanalyzable character of the sense of similarity. There are bold analysts who would not stickle at saying that the so-called visual sensations are thrown together into a class by the mere circumstance that they are acquired alike through the eye and the auditory sensations by the mere circumstance that they are acquired through the ear; the same applying to the other senses. This does not mean that if the visual sensations, in

themselves unchanged, should come through the ear they would be assimilated at once to the present auditory sensations and appear no longer in any wise alike; a statement which oversteps the bounds of sober discourse. It means that what we now experience and remember as visual sensations contain within them an element of feeling, muscular, perhaps, which is impartially the same in all. Now, if we can extend this to all the senses, it certainly seems to throw some illumination upon the problem of generic ideas; for these elements of feeling when detached might form the class-notions of the various sets of sensations on which they attend. But clearly the theory cannot stop short at those grand divisions that we call the senses. It must explain why dark blue and light blue, though really quite distinct colors, appear intrinsically more alike than dark blue and green; and so through a broad range of cases. Shall we suppose that, besides the attendant sensation which characterizes colors at large, there is a hierarchy of less general attendant sensations, distinguishing classes and sub-classes and still smaller groups until we come to the particular? It is a somewhat violent hypothesis; yet nothing short of it would seem to rescue us from the admission that there are classes of mental facts whose members have no one element in common.

And it is not only that the supposition strains our credulity; even on its own showing it does not really go far enough to help us here. I hope you will bear with me if I seem now and again to be too metaphysical in method. Metaphysics was certainly not nominated in the bond for this paper; but one can no more get a pound, nay an ounce, of strictly accurate psychology without a few drops of what some will call metaphysics than Shylock his covenanted flesh without blood. Either these common elements, in the case of colors, for instance, can be disengaged from the special elements with which they are ordinarily blended, or they cannot. If they cannot, we cannot form with them a pure generic image. If they can, then we ought to be able to imagine the residual quality of red or of green, apart from the 'common element' which allies it to the rest of its

class; and we ought to find that, thus imagined, it no longer shows likenesses or unlikenesses. For it would seem to be clean against psychological analogy to hold that we could extricate and isolate the common elements and yet not the special ones. But manifestly no such disappearance of likenesses ever takes place. It is a disappearance we should be quite at a loss to conceive.

I know that there are other replies that can be made to my objections, as well as other objections to this theory that could be advanced, but I believe the reply to be fallacious and the other objections for our present purpose needless. So far—perhaps too long—I have thought it necessary to dwell upon this conception by way of clearing ground. Having pointed out what seems to me its fatal weakness, I must pass on. In this brief compass I cannot attempt to prove my suggestions to the bitter end; I can do but a little more than lay them before you.

If I am right, then, this last resource of conceptualism is unable to sustain that doctrine, which accordingly must fall to the ground. That is to say, the tenet that a single mental state can by its content represent what are called the abstract qualities common to a class, cannot stand. But those who are perplexed by this outcome forget that there is another sense in which a mental state can represent a class. It can represent a class by its function, by the consequences it produces; not by what it is, but by what it does. An image with particular content can *do duty* as the representative of a class. By its psychical connections, in a word by its associations, it can prompt the right action towards the members of the class and inhibit any false thoughts about them.

Consider some of the critical cases. I assert of an object that it is red: 'The book is red.' The word 'the' implies that certain peculiarities of the book, perhaps its size, shape and position, are already known to you; but it is out of sight, and I am informing you of its color. Now, some psychologists would tell us that in thinking my sentence, 'the book is red,' you are affixing in thought to an object in part previously known an abstract property or 'universal'

also previously known. This is pure conceptualism. Others—some of the older Associationist schools, to wit—hold that we are seeing the book with the mind's eye, as similar to imagined red objects and as contrasted with imagined objects of another color. Judgment they regard, in accordance with the alleged psychological law of relativity, as involving comparison and contrast. But what shall hinder us from supposing—so far in agreement with the fine analysis of judgment contained in Professor Benno Erdmann's *Logik*—that we simply fill out our previous vaguer image of the book by imagining it as red—that shade of red which the word, used in connection with the word book, first calls to our mind. Previously we had conceived it with a tolerably specific size and shape, but the color was vague, dull and shifting. Now we conceive it a distinct red. The shade of red may need correction when we see the book, but meantime we have in such wise completed our image that we can use the word red of the book with security,—enable a servant, perhaps, to distinguish and bring it, or a child to avoid touching it. And this is clearly a typical case.

Thus, as regards a multitude of instances, we have got rid of the psychological abstract altogether, and have seen that the particular image, though perhaps imperfectly resembling the object in point of content, yet duly performs the function of guiding our action towards it, and of summoning at need the correct word. I may remind you that the word 'book,' as well as the word 'red,' called up a perfectly concrete, though probably a blurred or fragmentary or shifting, vision. The other cases show no essentially different process. Suppose I employ an abstract or generic term at large, as one may say. Suppose I ask: 'Do you prefer red or blue in bookbinding?' Before answering you compare in imagination a series of particular colors associated with the word red with a series of particular colors associated with the word blue. It would be idle, even were it possible, to compare generic red with generic blue, for your preference may be only for one especial shade. Here you see my deliberation is guided aright, because the word red

and the word blue duly perform their function of calling up the two ranges of colors with which they are severally associated. My understanding of the question and my correct answer depend mainly on the function, not the content, of some of my ideas. Suppose, again, that I lay down 'a general proposition:' 'all bright-colored books are easily soiled.' As an acute friend of mine once remarked, the mental process is better expressed by saying, '*Any* bright-colored book is easily soiled.' For what does the hearer (and with some modifications the speaker) mentally do in the matter? He runs hastily over a few cases—of bright-colored books long kept. If the associations thus started bring up a contrary instance, an instance, let us say, of such a book remaining untarnished under much handling, the 'general proposition' is rejected; that is, it does not become a belief, and the associations of its words do not serve as guides. If, however, all the instances are conformable, it may become a belief. I submit that the word 'all' in such propositions has no specific mental content whatever; that its force is merely to set the mind running over instances of the connection of ideas suggested by the rest of the proposition, and to make the proposition stand or fall as a belief, according to the harmony or discord of that connection of ideas with any instance whatever that the mental quest may find. The word 'all'—I mean the word as a mental phenomenon, the perception or idea of its sight, sound or articulation—is there for its function and not for its content. Virtually any other content would serve just as well if its function were the same. "Nothing," says Professor James, in the chapter on Conception in his 'Principles of Psychology,' "can give us the thought of 'all the possible members' of a class," but "an altogether special bit of consciousness *ad hoc*." And this 'bit of consciousness' is a transitive feeling, a mere 'sense of our meaning;' it belongs to the 'fringe' of thought. But surely the smallness of this modest bit of consciousness does not excuse it from the obligation of being intelligible. Even such a very small 'bit' cannot, in the long run, escape the eye of the psychological inspector; and if it should turn out to be carrying concealed

on its little person all the contraband assumptions of an unanalytic Conceptualism, it would go hard indeed. Do not mistake my meaning. Indisputably such transitive forms of consciousness exist and have important functions. But their importance depends on their appointed sequel, on what they bring after them in the mental train; not on the inward significance and essence of their 'altogether special' selves. In content they may be the merest chaff and trumpery of consciousness—the feeling of some momentary tension of the skin or twitch of a muscle. Is it anything but what Mr. James so aptly terms 'the psychologist's fallacy' to suppose that such transitive feelings must needs foresee their sequel and consciously lead to it? Is not this a signal example of the confusion between function and content?[1]

I said that the universal proposition might become a belief. The psychological condition that we call belief affords as good a case as one could have to enforce the distinction I am making. The current theories as to the nature of belief fall naturally into three classes. Descartes and Spinoza, and in recent times Professor Bain and Professor Bergmann, would assimilate it to the phenomena of volition. J. S. Mill and Professors Brentano, James, and Windelband may be named among those who regard belief as a form of consciousness *sui generis*, and not to be analyzed. According to thinkers of both these classes, the distinguishing characteristic of belief is to be found in the *content* of the mental state so named.

But there has been at the same time an altogether different course of thought on the topic. James Mill analyzed assent as an 'indissoluble association.' When I regard a certain conjunction of phenomena as the actual conjunction,

[1] It has been convenient to take instances from a work certain to be familiar to all psychologists and serious students of psychology. Meanwhile it is as well to add that most of the chief theses of this paper are identical or allied with those of an article by Professor James on 'The Function of Cognition,' *Mind*, Vol. X, and of such passages in his 'Psychology' as that on pp. 267–70, Vol. I. Professor James has made acknowledgment to myself in his Presidential Address, published last March in this REVIEW, for 'reconfirming' by the paper now printed his 'sometime wavering opinion.' My gratification at finding that my endeavors have been so blessed is greatly heightened by the sense that out of a hundred debts that cannot in sum be paid or measured, I have in one case been able to render him his own.

it means that the phenomena maintain to my mental vision that especial relation unaltered whenever I think of the matter. But in his notes on James Mill's 'Analysis of the Human Mind,' John Stuart Mill pointed out that this is not invariably the case. I *believe* that the *World* building—to take a homely instance—rises there at the corner of Park Row and Frankfort street. Yet I can conceive it away. I can in imagination, if I so choose, station a Greek temple or a Gothic cathedral on the spot. Clearly, then, the association is not indissoluble. This answer seemed final. But a new theory has been formed out of the ruins of James Mill's which sustains attack more successfully, as I think, than any other in the field. I refer to that of Mr. Alfred Hodder, which it is to be hoped the author will soon be moved to publish By his account of the matter, a belief is a conceived conjunction of phenomena in which the elements can indeed be torn apart by an effort of will, but in which, if left to themselves, they remain in the same position; the degree of the belief being proportional to the tendency of the elements to maintain or, if deliberately separated, to resume this position. Compared with Mill's definition, this theory makes it, not an *indissoluble*, but a *spontaneous*, association of ideas. The cohesion of the conceived elements may be likened to the elasticity of a strap of india-rubber. The strap can indeed be drawn out surprisingly far; but once relax the effort and it flies back at once to its natural dimensions. So precisely with our case of belief. I can, by some exercise of will, fancy a temple or a cathedral where the *World* building now stands; but no sooner do I cease to put forth the voluntary exertion than the poetic vision vanishes, and the hard prose of the newspaper building stands there to the mind's eye as remorselessly as ever.

I hold that this analysis of belief withstands objection better than any other offered to us. I have heard it urged, for instance, on the other side, that though we believe the earth to revolve round the sun, yet so far from that being the form the relation spontaneously takes to our mind, it is only with the greatest pains that we can imagine anything

else but that the sun goes round the earth. But this argument is entirely ineffective. For the purposes of daily life, I do imagine the sun rising and setting over the broad earth, and that is at such moments my belief. But when I am thinking of the planetary balls rolling in space (the conception with which education has familiarized me), then I spontaneously fall to conceiving the ball I call the earth revolving round that I call the sun. The conception of daily life does duty for the concerns of daily life; but it gives place to a totally different conception the moment an astronomical point of view suggests itself. It is hard, no doubt, to think of the earth as a sphere revolving in space, hard to realize the astronomical view; but when I once fairly do this, the earth begins of itself as it were, in my imagination, to revolve round the sun.

If I am right in deeming this theory of belief the best working hypothesis in the field (much virtue in that 'if,' you will say, but after all I can do no better in my brief space than suggest lines of argument, not carry them fully out)—if this theory holds its ground against the others, you see it is a triumph of those who find the distinguishing mark of belief not at all in the content or matter of the ideas present, but wholly in their mode of behavior, as one may say,—in their influence in preserving each other in firm association; in fine, in their properties or *function*.

By way at once of pointing and of summing up what I have said about this confusion in the subject of abstract and generic ideas, let me take an exceptionally difficult case. In the extremely careful work of Professor Goswin Uphues, of Halle, recently published—*Die Psychologie des Erkennens*—he deals once more with the old puzzle as to how our conception of 'nothing' can at once be a real idea possessed of a genuine content, and yet truly represent nonentity. Even Professor Uphues' answer, scrupulously reasoned and precisely stated as it is, does not, to my thinking, deliver us from the ultimate difficulties of the problem. I cannot understand how we can have a genuine idea of nothing if the content of an idea is the only sign of what it represents. I do not see how an idea by its intrinsic character can accu-

rately portray—nothing. But if we may look to its *function* also, the explanation is plain. If I believe that a certain room has nothing in it, I think of the empty interior of it—the bare walls and clear floor; and the mind, or if you will the brain, is so *set*, as we say of a trap, that any thought of an object in the room is promptly negatived so soon as it presents itself. My belief that there is nothing in the room is not constituted by my mere picture of a vacant room, but by the spontaneous self-maintenance of that picture in my mind, and the instant rejection, that is, failure to coalesce with the picture, and hence usually disappearance, of any intruding thought of contents. But we have conceptions of a more absolute 'nothing.' Take the belief that before a certain time nothing existed in space. In that case the picture of space empty at that time comes before the mind, and our habitual tendency to imagine it filled is checked as often as it shows itself. It may be objected, however, that you cannot conceive space except by virtue of there being something in it; that a strictly empty space excludes even the blue or gray background of atmosphere or cloud. Well, I do not wish to prejudge the question how far the visual element is necessary, as this objection supposes it, to the notion of space, and so I pass on to the extremest instance of nonentity within the bounds of thought. Let the belief be that before a certain date there existed absolutely nothing, neither matter nor space, angel nor spirit—not even the *locus* of a world. The content of my thought, which was first the empty room and then the void space (supposing the latter content possible), now disappears completely. Does the possibility of conceptions or belief disappear with it? By no means. What we call our belief in such a thoroughgoing nonentity is simply an attitude of the mind in which it stands ready to down any rising image of existent things as at that time in the world. Our belief is a state of standing indisposition on the mind's part to entertain any ideas of a certain character. Here you see the verbal sign has (save its sight, sound, or articulation) no 'content,' no corresponsive mental image at all. Its content is swallowed up in its function. Do not tell me that this is 'the grin with-

out the cat' of popular fable. In truth it is, like the 'potential energy' of the physicist, a prophecy or promise—in this case a negative promise; namely, such a state of tendency in the mind as insures the prompt suppression of any intruding thoughts of a certain order.

Notice in all this how far one goes astray when one assumes that language in its *structure* is in any sort whatever the copy or the homologue of thought. In language the predicate comes after the subject; in thought they come together. In language they are connected by a copula; but there is no copula in thought. There are indeed no verbs in thought. In thought there are but pictures, painted in the pigments of the different senses—picture supplanting picture in endless substitution. 'Action' and 'activity' are but the names we give to certain sequences in the melting and merging pictures as they pass. To speak of an action of the mind or the Ego, of an activity of consciousness itself, when attempting the language of analytic psychology, is to use an inept and unhelpful metaphor. Psychological study impresses us ever afresh with the lesson that language is a mere system of signals, dependent for its form and order on the structure and convenience of a bodily organ. Were we Hydra-headed or had we a hundred tongues, words might go abreast, might group themselves in a thousand new fashions, and thus in some respects more nearly take the shape of thought. But the broadest disparities—such as those which this discussion has made prominent—would still remain. Not long ago I stood on the bridge of a vessel which was signaling a lighthouse. The captain signaled with Coston lights and Roman candles. Now, it would be no more absurd to suppose that the captain's ideas thus expressed were related together as the Coston lights and the Roman candles than to take the grammatical form of the proposition for an outward and visible copy of the order of thought.

I now come to the subject in which the confusion of content and function has worked the most subtle and far-reaching harm; I mean the psychology of perception. It is the function of what we call our sense-perceptions to stand for

certain objective facts,[1] to stand for them in such wise as to guide our action in deference to them. These sense-perceptions must so play their parts that we do not fall into a pit, or walk off a precipice into thin air, or knock our fragile heads against a wall. Now things are so ordered, as we believe (let us not pry into the philosophical reasons of it here) that our perceptions do us this service by *resembling* the objective facts. To be sure it is common to make exceptions to this in certain items, color, taste, odor, etc.—but lay all that here aside. The pertinent fact is this: we find it by many assumed that this function must be *inscribed* as it were in the content of the perceptions themselves; that they must exhibit a conscious intent to represent outer objects as a portrait represents a man. In the portrait's case, of course, it is we who, looking on the man and on the painting, ascribe to the one the function of representing the other. But imagine if you can a portrait which shall be itself visibly possessed with the purpose of being like some person, and you have the parallel of what these thinkers ascribe to certain mental states. This is what I call looking for the function in the content; and I regard it as a forlorn quest due to a confusion. Those experiences that we name color and touch and resistance and the like are veritably there, and they guide our daily living; but they do not—touch and resistance no more than color—set up the absurd pretension of getting out of themselves or profess to reveal anything but their own intrinsic form and character. How we can know, if this be true, that they answer in some sort to a world beyond them is a question whose answer, plain and sufficient as I conceive it to be, does not belong to a psychological inquiry.

I know it was maintained yesterday[2] that the whole of this topic of our perceptive knowledge is outside the pale of psychology and lies on philosophical land. Professor

[1] I may not detain the reader for any metaphysical amplification or defence of these insufficient terms. But it will not be wholly useless to avail myself of the broad shelter of 'idealism'—to mention that my view is among those covered by that somewhat vague and varying name.

[2] See Professor G. S. Fullerton's Address on 'The Psychological Standpoint,' published in this REVIEW, Jan. 1894.

Fullerton, as you remember, erected a tall fence between the two, and gave us a concrete example of the relentlessness with which trespassers will be prosecuted. Well, definitions and exclusions are easy. I shall not make a new fence, and return railing for railing, as Dr. Arnold once said, by way of proving that the problem of perception belongs to psychology and not to metaphysics. I know that it belongs to both spheres. Psychology includes all mental facts, and if certain forms of consciousness purport to tell us of other things outside themselves, the fact of their so purporting is a mental fact and lies manifestly within the purview of psychology. I believe it, however, not to be a fact, but one of the most singular fictions in the history of thought. To my thinking this 'self-transcendence,' 'transubjective reference' and all the handsome phrases devoted to its service find no better warrant in introspection than they find countenance in logic. Obviously, I cannot stop to prove this in the dwindling residue of my time. I can only remark that if I am right (again an *if* of much virtue) we have here the source of certain modes of conceiving mental fact which have spread confusion through the terminology and thought of our science. For example, if we confound the function and the content of a knowing mental state, if we read the function into the content and say the mental state *means to know*, then immediately we must conceive of the mental state as an act, or an act with its results. Our would-be scientific language insensibly falls into metaphors taken from speech or painting in order to describe it, and says that it *reports* or *pictures* things out of consciousness. Then we grow used to speaking of the pictures on one hand and the picturing process on the other, as though the mind consisted of a stereopticon and a screen on which views are cast. We distinguish the thinking and the thing thought, or the feeling and the quality felt, or the thought and its mental object (which is 'what the thought thinks') or, with certain Germans, *das Vorstellen* and *das Vorgestellte;* all these terms meaning to refer to psychological and none of them to physical facts. And then, after all, as we look upon consciousness it seems to present a single and not a double face; and we proceed

to say, perhaps, with Professor Benno Erdmann: only *das Vorgestellte* is immediately given; the *Vorstellensvorgang* is inferred. Or we say with Professor James: only the objects are immediately given; the feelings themselves, the states of consciousness, might even conceivably be doubted. I submit that all this cleavage and distinction, together with these wonderful consequences, vanishes utterly away when we cease mixing content with function. For then we see, if I may for the moment use terms I reject, that the feeling as distinguished from the felt qualities is indeed an out-and-out fiction, and that the *Vorstellensvorgang* is falsely inferred. There is neither mental 'object' ideally projected from a 'state of consciousness' nor 'state of consciousness' endowed with the property of projecting it. State of consciousness and mental object, idea and content, are one.

THE ORIGIN OF A 'THING' AND ITS NATURE.[1]

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The present growing interest in genetic problems, as well as the current expectation that these discussions may render it necessary that certain great beliefs of our time be overhauled—these things make it important that a clear view should be reached of the sphere of inquiry in which questions of origin may legitimately be asked, and also just what bearing their answer is to have upon the results of the analytic study of philosophy.

We already have, in several recent publications, the inquiry opened under the terms 'origin vs. reality'—or, in an expression a little more sharp in its epistemological meaning, 'origin vs. validity.' I should prefer, in the kind of inquiry taken up in this paper, to give a wider form to the antithesis marked out, and to say 'origin vs. nature': meaning to ask a series of questions all of which may be brought under the general distinction between the 'how' of the question: how a thing arose or came to be what it is; and the 'what' of the question: what a thing is.

Well, first, as to 'what.' Let us see if any answer to the question 'what is it?' can be reached, adequate to our needs, in any case of genetic inquiry. It seems that the philosophy of to-day is pretty well agreed to start analysis of a thing inside of the behavior of the thing. A 'thing' is first of all so much observed behavior. Idealists pass quickly over the behavior, it is true; it is too concrete, too single, for them: it is not to them a thing, but a 'mere thing.' But yet they do not any longer allow this 'mereness' to offend them to the extent of drawing them off to other fields of exploration

[1] Notes presented to the Princeton Psychological *Seminar* in May, 1895, slightly revised.

altogether. They try to overcome the 'mereness' by making it an incident of a larger fullness: and the 'implications' of the thing, the 'meaning' of it 'in a system'—this shows up the mereness, both in its own insignificance and in its fruitful connection with what is universal.

So we may safely say of the idealist, that if he get a doctrine of a 'thing,' it must, he will himself admit, not be of such a thing that it cannot take on the particular form of behavior which the one 'mere thing' under examination is showing at the moment. There must, in short, be no contradiction between the 'real thing' and the special instance of it which is found in the 'mere thing.'

He, the idealist, therefore, is first of all a phenomenist in getting his doctrine of the real; the 'what' must be, when empirically considered, in some way an outburst of behavior.

Now the idealist is the only man, I think, of whom there is any doubt in the matter of this doctrine of behavior, except the natural realist, who comes up later. Others hold it as a postulate since Lotze, and later Bradley, did so conclusively show the absurdity of the older uncritical view which held, in some form or another, what I may call the 'lump' theory of reality. A thing can not be simply a lump. Even in matter—so we are now taught by the physicists—there are no lumps. To make a thing a lump—not to cite other objections to it—would be to make it impossible that we should know it as a thing. So all those doctrines which I have classed as other than idealistic accept, and have an interest in defending, the view that the reality of a thing is presented in its behavior.

So setting that down as the first answer to the 'what' question, we may profitably expand it a little. The more we know of behavior of a certain kind, then the more we know of reality, or of the reality, at least, which that kind of behavior is. And it is evident that we may know more of behavior in two ways. We may know more of behavior because we take in more of it at once; this depends on the basis of knowledge we already have—the relative advance of science in description, explanation, etc., upon which our interpretation of the behavior before us rests. In the behavior of a

bird which flits before him, a child sees only a bright object in motion; that is the 'thing' to him. But when the bird flits before a naturalist, he sees a thing whose behavior exhausts about all that is known of the natural sciences. Yet in the two cases there is the 'thing,' in just about the same sense.

When we come, farther, to approach a new thing, we endeavor, in order to know what it is, to find out what it is doing, or what it can do in any artificial circumstances which we may devise. Just as far as it does nothing, or as far as we are unable to get it to do anything, just so far we confess ignorance of what it is. We can neither summon to the understanding of it what we have found out about the behaviour of other things, nor can we make a new class of realities or things to put it in. All analysis is just the finding out of the different centers of behavior which a whole given outburst includes. And the whole, if unanalyzable to any degree, is itself a thing, rather than a collection of things.

But the second aspect of a thing's reality is just as important. Behavior means in some way change. Our lump would remain a lump, and never become a thing, if, to adhere to our phenomenal way of speaking, it did not pass through a series of changes. A thing must have a career; and the length of its career is of immediate interest. We get to know the thing not only by the amount of its behavior, secured by examining a cross-section, so to speak, but also by the increase in the number of these sections which we are able to secure. The successive stages of behavior are necessary in order really to see what the behavior is. This fact underlies the whole series of determinations which ordinarily characterize things, such as cause, change, growth, development, etc., as comes out farther below.

The strict adherence to the definition of a thing in terms of behavior, therefore, would seem to require that we waited for the changes in any case to go through a part at least of their progress—for the career to be unrolled, that is, at least in part. Immediate description gives, as far as it is truly immediate, no science, no real thing with any richness of content; it gives merely the snap-object of the child. And

if this is true of science, of every-day knowledge of things, to live by, how much more of the complete knowledge of things desiderated by philosophy? It would be an interesting task to show that each general aspect of the 'what' in nature has arisen upon just such an interpretation of the salient aspects presented in the career of individual things in nature. But this would be to write a large and most difficult chapter of genetic philosophy.

Our second point in regard to the 'what,' therefore, is that any 'what' whatever is in large measure made up of judgments based upon experiences of the 'how.' The fundamental concepts of philosophy reflect these categories of origin, both in their application to individuals—to the 'mere thing'—and also in the interpretation which they have a right to claim: for they are our mental ways of dealing with what is 'mere' on one hand and of the final reading of reality which philosophy makes its method. Of course the question may be asked: How far, origin? That is, how far back in the career of the thing it is necessary to go to call the halting-place 'origin.' This we may well return to lower down; the point here is that origin is always a reading of part of the very career which is the content of the concept of the nature of the thing.

Coming now closer to particular instances of the 'what,' and selecting the most refractory case that there is in the world, let us ask these questions concerning the *mind*. I select this case because, in the first place, it is the case urgently pressing upon us; and, second, because it is the case in which there seems to be, if anywhere, a gaping distinction between the 'what' and the 'how.' Modern evolution claims to discuss the 'how' only, not to concern itself with the 'what;' or, again, it claims to solve the 'what' entirely by its theory of the 'how.' To these claims what shall we say?

From our preceding remarks it seems evident that the nature of mind is its behavior generalized; and, further, that this generalization necessarily implicates more or less of the history of mind; that is, more or less of the career which

discloses the 'how' of mind. What further can be said of it as a particular instance of reality?

A most striking fact comes up immediately, when we begin to consider mental and with it biological reality. The fact of growth, or to put the fact on its widest footing, the fact of *organization*. The changes in the external world which constitute the career of a thing, and so show forth its claim to be considered a thing, fall under some very wide generalizations, such as those of chemistry, mechanics, etc.; and when the examination of the thing's behavior has secured its description under these principles in a pretty exhaustive way, we say the thing is understood. But the things of life, and the series of so-called organic changes which unroll its career, are not yet so broadly statable. When we come to mind, again, we find certain pretty well made out generalizations of its behavior. But here, as in the case of life, the men who know most have not a shadow of the complacency with which the physicist and the chemist categorize their material. It is for this reason, I think, in part, that the difference between the two cases gets its emphasis, and the antithesis between origin and nature seems so necessary in one case while it is never raised in the other. For who ever heard a natural science man say that the resolution of a chemical compound into its elements, thus demonstrating the elements and law of the origin of the 'thing' analyzed, did not solve the question of its nature, as far as science can state a solution of that question?

But we can not say that the whole difference is one of greater modesty on the part of the psychologists. The facts rather account for their modesty. And the prime fact is one formulated in more or less obscurity by many men, beginning with Aristotle: the fact, namely, that organization considered as itself a category of reality never reaches universal statement in experience. To confine the case at first to vital phenomena, we may say that to subsume a plant or animal under the category of organization is to make it at once to a degree an X: a form of reality which, by right of this very subsumption, predicts for itself a phase of behavior as yet unaccomplished—gives a prophecy of more career, as a fact,

but gives no prophecy (apart from other information which we may have) of the new phase of career in kind. Every vital organization has part of its career yet to run. If it has no more career yet to run, it is no longer an organization: it is then dead. It then gets its reality exhausted by the predication of the categories of chemistry, mechanics, etc., which construe all careers retrospectively. A factor of the biological and mental categories alike is just this element of what I have called elsewhere 'Prospective Reference.'[1] In biology it is the fact of 'Accommodation:' in psychology it is the same fact found in all cases of Selection—most acute in Volition.

And it does not matter how the content in any particular filling up of the category may be construed after it takes on the form of accomplished fact—after, *i. e.*, it becomes a matter of 'retrospect.' All constructions in terms of content mean the substitution of the retrospective categories for those of prospect; that is, the construction of an organization after it is dead, or—what amounts to the same thing—by analogy with other organizations which have run down, or died, in our experience. Suppose, for example, we take the construction of the category of Accommodation, in each particular instance of it, in terms of the ordinary biological law of natural selection—an attempt made by the present writer under the statement of so-called 'Organic Selection'[2]—and so get a statement of how an organism actually got any one of the special adaptations of its mature personal life. What, then, have we done? I think it is evident that we have simply resorted to the 'retrospective' reference; we have changed our category in the attempt to get a concrete filling for a particular case *after it has happened*. To adopt the view that the category of organization can be in every case filled up with matter, in this way, does not in any sense destroy the prospective element in the category of organization; for the psychological subtlety still remains in mind in the

[1] See my *Mental Development: Methods and Processes* (Macmillan & Co.), Chaps. VII, XI.

[2] In accordance with which the organism's new accommodations are selected out of movements excessively produced under pleasure-pain stimulation. *Ibid.* pp. 174 f.

doing of it, either that the event must be awaited to determine the outcome, and that I am agreeing with myself and my scientific friends to wait for it, or that we are solving this case by others for which we did wait. A good instance of our mental subtleties in such cases is seen in the category of 'potentiality,' considered lower down. The extreme case of the reduction of the categories of prospective reference to those of retrospect, is evidently the formula for probabilities. I do not see how that formula can escape being considered a category of retrospect, applied to material which does not admit of any narrower or more special retrospective formulation.

Now the inference from this is that our predicate 'reality,' in certain cases, is not adequately expressed in terms of the experienced behavior of so-called real content. The very experience on the basis of which we are wont to predicate reality testifies to its own inadequacy. I see no way to avoid the alternatives that either the notion of reality does not rest upon experiences of behavior, or that the problematic judgments based upon those experiences of progressive organization which we know currently under the term development, are as fundamental to these kinds of reality as are those more static judgments based on history or origin.

It may be well, in view of the importance of this conclusion, to see something more of its bearings in philosophy. The historical theories of design, or teleology in nature, have involved this question. And those familiar with the details of the design arguments pro and con will not need to have brought to mind the confusion which has arisen from the mixing up of the 'prospective' and 'retrospective' points of view. Design, to the mind of many of the older theistic writers, was based upon relative unpredictibility—or better, infinite improbability. Such an argument looks forward: it is reasoning in the category of organization, but under the 'prospective' reference. The organization called mental must be appealed to. What, was asked, is the probability of the letters of the Iliad falling together so as to read out the Iliad? The opponents, on the other hand, have said: Why is not the Iliad combination as natural as any other?

One combination has to happen; what is to prevent this? If a child who cannot read should throw the letters, the Iliad combination is no more strange to him than any other. These men are reasoning in the retrospective categories. They are interpreting facts. The fault of the latter position is that it fails to see in reality the element of higher organization which the whole series when looked at from the point of view of the real Iliad requires. What would really happen, if the child should throw the Iliad combination, would be that nature had produced a second time a combination once before produced (in the mind of Homer, and through him in ours) without fulfilling all the other combinations—an infinite number—which have a right to be fulfilled before the Iliad combination be reproduced. But this added element of organization needed to bring nature into accord with thought and which the postulate of design makes in reaching a Designer—this is not needed from the mere historical or reprospective examination of the facts. In other words, if the opponents of design are right in holding to a complete reduction of organization to retrospective categories, they ought to be able to say just as definitely that the Iliad combination will happen in a certain number of throws, as they are to say afterwards that it has happened.

The later arguments for design, therefore, which tend to identify it with organization, and to see in it, so far as it differs from natural law, simply a harking forward to that career of things which is not yet unrolled, but which when completely unrolled will be a part of the final statement of origins in terms of natural law—this general view has the justification of as much criticism as has now been stated.

And, further, it is clear that the two opposed views of adaptation in nature are both genetic views — instead of being, as is sometimes thought, one genetic (that view which interprets the adaptation after it has occurred), and the other analytic or intuitive (that view which seeks a beforehand construction of design). The former of these is usually accredited to the evolution theory; and properly so, seeing that the evolutionist constantly looks backward. But the other view, the design view, is equally genetic. For

the category of higher or mental organization by which it proceeds is just as distinctly an outcome of the movement or drift of experience toward an interpretation of career in terms of history. Teleology, then, when brought to its stronghold, is a genetic outcome, and owes what force it has to the very point of view that its most fervent advocates—especially its theological advocates—are in the habit of running down. The consideration of the stream of genetic history itself, no less than the attempt to explain the progress of the world as a whole, its career, leads us to admit that the real need of thinking of the future in terms of organization is as great as the need of thinking of the past in terms of natural law. The need of so-called mental organization or design is found in the inadequacy of natural law to explain the further career of the world, and its past career also, as soon as we go back to any place in the past and ask the same question there. It would be possible, also, to take up the last remark for more thought, and to make out a case for the proposition that the categories of 'retrospective' thinking also involve a strain of organization—a proposition which is equivalent to one which the idealists are forcibly urging from other grounds and from another point of view. Lotze's argument to an organization at the bottom of natural causation has lost nothing of its power. Viewed as a category of experience, I am unable to see the force of the assumption tacitly made by the Positivists, and as tacitly admitted by their antagonists, that causation is to be ultimately viewed entirely under such retrospective constructions as 'conservation of energy,' etc. Such constructions involve an endless retrospective series. And that is to say that the problem of origin is finally insoluble. Well, so it may be. But yet one may ask why this emphasis of the 'retrospective,' which has arisen in experience with just the basis of experience that the 'prospective' also has? It may be a matter of taste; it may be a matter of 'original sin.' But if we go on to try to unite our categories of experience in some kind of a broader logical category, the notion of the Ultimate must, it would seem, require both of the aspects which our conception of reality includes; the 'prospective' no less than the 'retro-

spective.' Origins must take place continually as truly as must sufficient reasons. The only way to avoid this is to say that reality has neither forward nor backward reference. So say the idealists in getting thought which is not in time. But be that as it may, we are dealing with experience—though for myself, I must say, thought which looks neither backward nor forward is no thought at all.

Another subtlety might raise its head in the inquiry whether in their origin all the categories did not have their 'natural history.' If so, it might be said, we are bound, in the very fact of thinking at all, to give exclusive recognition to the historical aspect of reality. But here is just the question: does the outcome of career to date give exhaustive statement of the idea of the career as a whole? There would seem to be two valid objections to it. First, it would be, even from the strictly objective point of view, the point of view of physical science, to construe the thing mind entirely in terms of the behavior of its stages antecedent to the present: that is entirely in terms of descriptive content, by use of the categories of retrospective interpretation. And, second, it does not follow that because a mental way of regarding the world is itself a genetic growth, therefore it is an illusory way. Let me explain these two points a little.

1. A chemist seemed justified in looking at atmospheric air, as explained by the formula for a mixture of nitrogen and hydrogen, for the reason, and this is his practical test, that the behavior of air confirms that view. His confidence in his statements of history can only be justified on the ground that present history never contradicts it. But as soon as a new experiment showed that new behavior may be different, and may contradict the reports of history, he looks for a new thing, argon—new in the sense, of course, that the historical manifestations of the kind of reality in so-called air had never before brought it to recognition. In other words, the nature of air had been stated in terms of oxygen and nitrogen; but he now sees that the statement founded on what was known of origin—and that is what origin means in all these discussions—was inadequate. This would seem to admit, however, that if the problem of origin could be really exhausted,

that of nature would be exhausted too; and no doubt it would. But it is a corollary from the second point of objection, soon to be made, that the problem of origin can never be exhausted, even by philosophy, without an appeal to other than the historical or retrospective categories.

But before I pass on to the second objection to the position that a thing which is admitted to have had a natural history must have its interpretation adequately given in that history, and that this applies also to the very categories by the use of which its denial is effected—before going farther I may point out an extreme case of the main position as sometimes argued by evolutionists. If, it may be said, the mind has developed under constant stimulations from the external world, and if its progress consists essentially in the more and more adequate representation in consciousness of the relations already existing in the external world, then it follows that these internal representations can never do more than reflect the historical events of experience. Consciousness simply testifies again to the real as it has been testified to her before. How, then, can there be any such thing as a phase of reality not subject to plain statement under natural law?[1]

This a very common objection to all thorough-going statements of mental evolution. It rests on the mistaken view, just pointed out, that a statement of the historical career of a thing can ever be an adequate statement of its nature; in other words, that the origin of the categories of thought can tell what these categories will do—what their function and meaning is in the general movement of reality. Consciousness is entitled to a hearing in terms of its behavior solely. Its behavior, attitudes, etc., represented by

[1] It is this supposed necessity that leads Mr. Huxley to hold that evolution cannot explain ethics, *i. e.*, the supposed necessity that the validity of ethical values must be adequately found in the terms of their origin; for, says he, the pursuit of evil would have as much sanction as that of good, for both are in us, and they would have the same origin (*Evol. and Ethics*, esp, p. 31). But to say, as we do, that the appeal made by the word 'ought' is a 'prospective' appeal, as opposed to the description of the 'is,' which is 'retrospective,' does not require us to say that the impulse to recognize either is not a product of evolution. My discussion of Prof. Royce's attempt (*Int. Jour. of Eth.*, July, 1895) to show the psychological origin of the antithesis between 'ought' and 'is,' may be referred to (*Int. Jour. of Eth.*, Oct., 1895).

'prospective' thought are there just as its behavior represented by its history is there. Who would venture to say that consciousness of a relation in nature is in no sense a different mode of behavior from the relation itself in nature? The real point is in what I have already tried to put in evidence: that such a construction involves the assumption that reality in its movement defines all her own changes in advance of their actual happening. The very series of changes which constitute the basis in experience for the growth in consciousness of the category of change are the basis also for the new aspects of reality (say consciousness) which are held to be only a putting in evidence of the relations already existing in nature. If consciousness is no new thing—on our behavior-definition of thing—then knowledge of the historical movement of reality must be not at all different from the movement which has led up to knowledge. The discovery of the principle of evolution, for example, is not a new event added to the fact that the series evolving was there to be discovered!

But I may be even more concrete. I have recently developed a view of mental development which not only makes each stage of it a matter of legitimate natural history, but goes on to say that the one method of motor adaptation is by imitation. What could be a more inviting field for the criticism: imitation is mere repetition. How can anything new come out of imitation? Not only is consciousness merely repeating the relationships already there in nature, but the development of consciousness itself is merely a series of repetitions of its own acts. I have had this criticism already; especially with reference to volition. How, it is asked, can anything new be willed if volition is in its origin only imitation become complex?

I reply in a way to make concrete what has been said immediately above. The counter question may be put: why can not anything new come out of imitations? Why may not the very repetition be the new thing, or the condition of it? To say not is to say that by looking at the former instance, the historical, after its occurrence, you can say that that occurrence fully expressed mental behavior. On the

contrary, the prospective reference gained by the imitation may bring out something new; the repetition may be just what is needed to bring an important stage in the career of mental reality. In itself, of course, an imitation is no more open to the objection we are considering than any other kind of mental behavior; but it seems to be more so, because it emphasizes the very point that the current objection to natural history hits upon, *i. e.*, that it makes the mind only a means of reinstatement of relations already existing in nature, and then makes that the explicit method of mental history.

2. The second answer to the view now being criticised may be put in some such way as this. It does not follow that because a product—one of the categories of organization, such as design, the ethical, &c.—is itself a matter of gradual growth, its application to reality is in any way invalidated. A category must be complete, ready-made, universal, without exceptions, we are told, in order that its application to particular instances be justified. But I fail to see the peculiar and mysterious validity supposed to attach to an intuition because whenever we think by it we allow no exceptions. Modern critiques of belief and modern theories of nervous habit have given us reasons enough for discarding such touch-stones as 'universality' and 'necessity.' And modern investigations into the race development of beliefs have told us how much better an aspect of reality really is because at one time people insisted in thinking in a certain intuitive way about it. The whole trouble, as I think, with the intuitional way of thinking is curiously enough that fallacy which I have pointed out as being a favorite one of the evolutionists. The evolutionists say that an intuition is of no value when construed prospectively, *i. e.*, as applying to what 'must be' beyond 'what is': it gets all its content, and all its force, from experience. Therefore, all reality is to be construed retrospectively, and no 'thing' is possible except as accounted for as an evolution from historical elements. True after things have happened, it nevertheless fails by thinking career all finished. Why may not experience produce in us a category whose meaning is prophetic? On the other hand, here come the intuitionists and oppose the evo-

lutionists in this way. They say: no thing is possible except as in some way evidenced for. The intuitions are universal and necessary. As such their evidence can not be found in experience. To admit that they had developed would be to admit that their evidence could be found in experience. Consequently they carry their own evidence and their own witness is all the evidence they have. The fallacy again is just the assumption that reality is finished, that categories of retrospective reference exhaust the case. That the series of events which are sufficient ground for the origin of the category should also be sufficient evidence of its validity. That there is a sharp contradiction, therefore, between a doctrine of derivation from experience (which is inadequate as evidence) and application beyond experience. But when we come to see that the categories of prospective thought are equally entitled to application with those of retrospect, we destroy the weapon of evolution to hurt the validity of mental utterances, and at the same time knock out the props upon which the intuitionist has rested his case.

The case stands with mental facts, to sum up, just about as it does with all other facts. An event in nature stays what it is until it changes. So with an event or a belief or any other thing in the mind of the race. It stays what it is until it has to change. Its change, however, is just as much an element in reality as lack of change is; and the weakening of a belief like any other change is the introduction of new phases of reality. A doctrine which holds to intuitions which admit of no prospective exceptions, no novelties, seems to me to commit suicide by handing the whole case over to a mechanical philosophy; for it admits that all validity whatever must be cut from cloth woven out of the historical and descriptive sequences of the mind's origin.

Our conclusions so far may be summed up tentatively in certain propositions as follows:

1. All statements of the nature of a 'thing' get their matter mainly from the processes which they have been known to pass through: that is, statements of nature are for the most part statements of origin.

2. The statements of origin, however, never exhaust the

reality of a thing; since such statements cannot be true to the experiences which they state unless they construe the reality not only as a thing which has had a career but also as one which is about to have a career: for the expectation of the future career rests upon the same historical series as the belief in the past career.

3. All attempts to rule out prospective organization or teleology from the world would be fatal to natural science, which has arisen by provisional interpretations of just this kind of organization: and also to the historical interpretation of the world found in the evolution hypothesis; for the category of teleology is but the prospective reading of the same series which, when read retrospectively, we call evolution.

4. The fact of natural history of any thing, and more especially of mental products, ideas, intuitions, &c., is no argument against its validity or worth as having application beyond the details of its own history; since, if so, then a natural history series can produce nothing new. But that is to deny the existence of the fact or idea itself, for it is a new thing in the series in which it arises.

5. All these points may be held together in a view which gives each mental content a two-fold value in the active life. Each such content begets two attitudes by its function as a genetic factor in the progressive development of the individual. As far as it fulfils earlier habits it begets and confirms the historical or retrospective attitude, as far as it is not entirely exhausted in the channels of habit, so far it begets the expectant or prospective attitude.

There are one or two points among many suggested by the foregoing which it may be well to refer to—selected because uppermost in my own mind. It will be remembered that in speaking of the categories of organization as having prospective reference, I adduced instances largely drawn from the phenomena of life and mind, contrasting them somewhat strongly with those of chemistry, physics, &c. The use afterwards made of these categories now warrants us in turning upon that distinction, in order to see whether our main results hold for the aspects of reality with which these sciences deal as well. I have intimated above in passing that

the other categories of reality, such as causation, mechanism, are really capable of a similar evaluation as that given to teleology. This possibility may be put in a little stronger light.

It is evident, when we come to think of it, that all organization in the world must rest ultimately on the same basis; and the recognition of this is the strength of thorough-going naturalism and absolute idealism alike. The justification of the view is to be made out, it seems to me, by detailed investigation of the genetic development of the categories. The way the child reaches his notion of causation, for example, or that of personality, is evidence of the way we are to consider the great corresponding race-categories of thought to have been reached: and the category of causation is, equally with that of personality, or that of design, a category of organization. The reason that causation is considered a cast-iron thing, implicit in nature in the form of 'conservation of energy,' &c., is that in the growth of the rubrics of thought certain great differentiations have been made in experience according to observed aspects of behavior; and those events which exhibited the more definite, invariable aspects of behavior have been put aside by themselves; not of course by a conscious convention of man's, but by the conventions of the organism working under the very method which we come—when we make it consciously conventional—to call this very category of organization. What is conservation but a kind of organization looked at retrospectively and conventionally? Does it not hold simply because my organism has made the convention that only that class of experiences which are 'objective' and regular and habitual to me shall be treated together, and so shall give rise to such a regular mental construction on my part?

But the tendency to make all experience liable to this kind of causation is an attempt to undo nature's convention—to accept one of her results, which exists only in view of a certain differentiation of the aspects of reality, and apply this universally, to the subversion of the very differentiation on the basis of which it has arisen. The fact that there is a class of experiences whose behavior issues in such a purely

historical statement and arouses in me such a purely habitual attitude, is itself witness to a larger organization—that of the richer consciousness of expectation, volition, prophecy. Otherwise conservation could never have got for itself abstract statement in thought.

The reason that the category of causation has assumed its show of importance, is just that which intuitionist thinkers urge; and another historical example of confusion due to their use of it may be used for illustration. Causation is about as universal a thing—in its application to certain aspects of reality—as could be desired. And we find the men of this school using this fact to reach a certain statement of theism. But they then find a category of 'freedom' claiming the dignity of an intuition also; and although this comes directly in conflict with the uniformity ascribed to the other, nevertheless it also is used to support the same theistic conclusion. The two arguments read: (1) an intelligent God exists because the intelligence in the world must have an adequate cause, and (2) an intelligent God exists because the consciousness of freedom is sufficient evidence of a self-active principle in the world, which is not caused. All we have to say, in order to avoid the difficulty, is that any mental fact is an 'intuition' in reference only to its own content of experience. Intelligence viewed as a natural fact, *i. e.*, retrospectively, has a cause: but freedom in its meaning in reality, *i. e.*, with its prospective outlook, is prophetic of novelties—is not adequately construed in terms of history. So both can be held to be valid, but only by denying universality to both 'intuitions' and confining each to its sphere and peculiar reference in the make up of reality.

Another thing to be referred to in this rough discussion concerns the more precise definition of 'origin.' How much of a thing's career belongs to its origin? How far back must we go to come to origin?

Up to this point I have used the word with a meaning which is very wide. Without trying to find a division of a thing's behavior into the present of it as distinguished from its history; I have rather distinguished the two attitudes of mind engendered by the contemplation of a thing, *i. e.*, the

'retrospective' attitude and the 'prospective' attitude. When we come to ask for any real division between origin and present existence we have to ask what a thing's present value is. In answer to that we have to say that its present value resides very largely in what we expect it to do; and then it occurs to us that what we expect it to do is no more or less than what it has done. So our idea of what is, as was said above, gets its content from what has been. But that is to enquire into its history, or to ask for a fuller or less full statement of its origin or career. So the question before us seems to resolve itself into the task of finding somewhere in a thing's history a line which divides its career up to the present into two parts; one properly described as origin, and the other not. Now, on the view of the naturalist pure and simple there can be no such line. For the attempt to construe a thing entirely in terms of history, entirely in the retrospective categories, would make it impossible for him to stop at any point and say 'this far back is nature and farther back is origin'; for at that point the question might be asked of him 'what is the content of the career which describe the thing's origin?'—and he would have to reply in exactly the same way that he did if we asked him the same question regarding the thing's nature at that point. He would have to say that the origin of the thing observed later was described by career up to that point; and is not that exactly the reply he would give if we asked him what the thing was which then was? So to get any reply to the question of the origin of one thing different from that to the question of the nature of an earlier thing, he would have to go still farther back. But this would only repeat his difficulty. So he would never be able to distinguish between origin and nature except as different terms for describing different sections of one continuous series of aspects of behavior. This dilemma holds also, I think, in the case of the intuitionist. For as far as he denies the natural history view of origins and so escapes the development above he holds to special creation by an intelligent Deity; but to get content to his thought of Deity he resorts to what he knows of mental behavior. The nature of mind then supplies the thought of the origin of mind.

To those who do not shut themselves up, however, to the construction of things in the categories of realized fact, of history, of 'retrospect,' the question of origin is a fruitful one apart from the statement of nature. For at any stage in the career of a thing the two methods of thought are equally applicable. When we ask how a thing originated, we transport ourselves back to a point in its career at which the 'prospective' categories got a filling not *at that stage* already expressed in the content of history. The overplus of behavior is said to have its origin then, even though afterwards the outcome be statable in the categories of retrospect which have *then been widened by this event*. For example, volition originates in the child at the point of its life at which certain conscious experiences issue out of old content, experiences which were not previously there, to the child, in whatever complications of content were there. But once arisen, the experience can be construed as a continuation of the series of events which make up mental history. To the Positivist and to the Intuitionist a sensational account of the genesis of volition, and to the intellectual Idealist an ideological account of it, rule volition out of reality just by the fallacy of thinking exclusively in retrospect; but the truth is to say "granted either account of its origin, it leaves philosophy still to construe it: for if we estimate volition from facts true before volition arose, the sources do not fully describe it; and if we wait to view it after it arises, then the full statement of career must include the widened aspects of behavior which the facts of volition afford."[1]

It is interesting also to note, as another case of application of this general distinction between the mental habits represented respectively by the terms 'prospective' and 'retrospective,' that it gives us some suggestions concerning the very obscure concept called potency or potentiality. This soi-disant concept or notion has been used by almost every conceivable shade of thought as the repository of that which is unexplained. Aristotle started the pursuit of this

[1] In the last number (Sept, 1895) of this REVIEW I criticised Professor Watson's view that the Absolute can be exhausted by our thought, *i. e.*, can be adequately expressed in terms of the organizations of content already effected.

notion and used it in a way which shed much light, it is true, upon the questions of philosophy concerned with change and organization; but his failure to give any analysis of the concept itself has been an example ever since to lesser men. It is astonishing that, with all the metaphysics of causation which the history of philosophy shows, there has been—that is to my knowledge—no thorough-going attempt to trace the psychological meaning of this category. How common it is to hear the expression, 'this thing exists, not actually, but potentially,' given as the end of debate, and accepted, too, as the end. I do not care to go now into a historical note on the doctrine of potentiality; it would be indeed mainly an exposition of a chapter of Aristotle's metaphysics with the refinements on Aristotle due to the logic of the schoolmen and the dogmatic of modern theology. It may suffice to say something of the natural history of the distinction between potential and real existence in the light of the positions now taken.

In brief, then, there are two aspects as we have seen under which reality must in all cases be viewed—the prospective and the retrospective. The retrospective, as has been said, is the summing up of the history which gives positive content to the notion of a thing considered as accomplished career. This aspect, it seems clear, is what is in view when we speak of real existence in contrast with potential existence. It is not indeed adequately rendered by the content supplied by retrospect, since the fact that the two predicates are held in mind together as both together applicable to any concrete developing thing, forbids us to construe real existence altogether apart from the fact that it has a farther issue in farther career. It is a great merit of Aristotle that he forbade just this attempt to consider the *dunamis* apart from the *energeia*. But, nevertheless, it is true psychologically that real existence is exhausted as a content-predicate with the backward aspect of the series of changes which give body to reality.

And it seems equally evident at first blush that potential existence is equally concerned with the prospective reference of the thought of things. That this is so is perhaps the one element in the notion of potency that all who use the word

would agree upon. But this is inadequate as a description of the category of potentiality. For if that were all, how would it differ from any other thought of the prospective? We may think of the future career of a thing simple in terms of time; that, we would probably agree, does not involve potentiality. A particular potency is confined to a particular thing, *i. e.*, to a particular series of events making up a more or less isolated career. If only the bare fact of futurity were involved, why should not any new unrolling of career be the potency of any thing indiscriminately?

This leads us to see that potency or potentiality, even when used in the abstract, is never free from its concrete reference. And this concrete reference is not that of conception in general, only or mainly; the concrete reference of conception generally is a matter of retrospect, *i. e.*, of the application of the concept to individual things, as far as such application has been justified by historical instances. Indeed, it is the very occurrence of the historical instances which has given rise to the concept, and it generalizes them.

So when we put ourselves at the point of view of the concrete, we have to ask what is actually meant by us when we say a thing exists potentially, over and above the mere meaning that the thing is to exist in the future. We have seen that one added element of meaning is that the thing which is to exist in the future is in some way tied down in its manifestations to something that already exists actually; it must be the potentiality of some one thing in order to be a potentiality at all. Now, how can this be?

Of course the ordinary answer is at once on our lips: the answer that the bond between the thing that is and the thing that is to be is the bond of causation. The potentiality is the unexpressed causal efficacy of the thing that is. But when we come to ask what this means, we find that we are hiding behind one of the screens of common sense. The very fact of cause, whatever bond it may represent from an ontological point of view, is at least a fact of career. The effect is a further statement of the career of the thing called the cause. Now, to say that the potency of a thing is its unexpressed causal power, is only to say that the thing has

not finished its career, and that is a part of the general notion of a thing. That fact alone does not in any way define the future career for us, except in the way of repetition of past career. We merely expect the thing to do what it has done before; not to become some new thing out of the old. In short, the category of causation is not adequate, since it construes all career retrospectively.

We have, therefore, two positions so far, saying (1) that every potency is the potency of a thing, and this means that it gets its content in some way from the historical series which that thing embodies; but (2) that it is something more than a restatement of any or all of the elements of the series thus embodied. Now, what else is there?

The remaining element in the category of potentiality involves, I think, a very subtle movement of the mind along the same distinction of the prospective from the retrospective. Briefly, the potentiality which I ascribe to a thing is my general expectation of more career in reference to it, with the added sense, based on the combined experiences of mine that the prospective does get a retrospective filling after it has happened, that the new career of the thing to which I ascribe the potency, although not yet unfolded, will likewise be capable of retrospective interpretation as further statement of the one series which now defines the thing.

In short, there are three elements or phases of consciousness in this matter: first, let us say, the general prospective element, the expectation that something will happen; second, the causation or retrospective element, the expectation that when it has happened it will be a consistent part of the history of the thing; and, third, the conscious setting back of my observation to the dividing line between these two points of view, and the contemplation of the thing under both of them—both as a present thing, and as a thing for what it will be when the future becomes present.

For example: I say that a tree expresses the potency or potentiality of the seed. This means three very concrete things. I expect the seed to have a future; I expect the future to be a tree—that is, a thing whose descriptive series is continuous with that already descriptive of the seed—and,

finally, I now look upon the seed as embodying the whole tree series now artificially present in my thought.

Of course, on the view of this paper the question of the ultimate origin of the universe may still come up for answer. Can there be an ultimate stopping-place anywhere in the career of the thing-world as a whole? Does not our position make it necessary that at any such stopping-place there should be some kind of filling drawn from yet antecedent history to give our statement of the conditions of origin any distinguishing character? It seems to me so. To say the contrary would be to do in favor of the prospective categories what we have been denying the right of the naturalist to do in favor if those of retrospect. Neither can proceed without the other. The only way to treat the problem of ultimate origin is not to ask it, as an isolated problem. Lotze says that the problem of philosophy is to require what reality is, not how it is made; and this will do if we remember that we must exhaust the empirical 'how' to get a notion of the empirical 'what,' and that there still remains over the 'prospect' which the same author has hit off in his famous saying, 'Reality is richer than thought.' To desiderate a what which has no how—this seems as contradictory as to ask for a how in terms of what is not. It is really this last chase of the 'how' that Lotze deprecates—and rightly.

Addenda. (1) *Further applications:* to the discussion of *freedom;* to the discussion of *ideals;* criticism of the general concept of *law* from this point of view; applications in *ethics* (*cf.* with Royce's distinction vs. 'world of description' and 'world of appreciation'); question of the notion of time (*i. e.*, is the distinction between the 'prospective' and 'retrospective' merely one of time, or does the notion of time find its genesis in this difference of mental attitude?)

(2) *References:* Ritchie, *Darwin and Hegel*, Chap. I; Royce, *Spirit of Modern Philosophy*, *in loc.* and *Int. Journ. of Ethics*, July 1895; Baldwin, *Mental Development: Methods and Processes*, Chaps. VII, XI, and *Int. Journ. of Ethics*, Oct. 1895.

SOME OBSERVATIONS ON THE ANOMALIES OF SELF-CONSCIOUSNESS.

(II.)

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I spent some months, a few years since, in pretty frequent and close intercourse with a young man who, though then certainly unknown in his inner life to any medical man, was a pretty highly pathological instance of the more metaphysical type of the malady of self-consciousness. He came for counsel as a young genius, willing to let me read endless manuscript productions of his own, including his diaries, which I was permitted to examine. He was disposed to get some advice about his intended career as a poet, as man of free soul, and as independent person generally. He was a man of twenty-four, in easy circumstances, uncontrolled by his parents, of fairly robust physical appearance, and, so far as I could guess, of generally good vegetative health—a man who had certainly so far been able to bear, without much physical inconvenience, the strain of a good deal of dissipation. No serious illnesses were admitted in his past since childhood. His appetite and sleep were reported as good; his emotional undertone, however hard you tested him, was one of pretty steady cheerfulness, even in the midst of his greatest perplexities; his social manners were gentle, and on the whole rather feminine in their kindliness, their plasticity, their somewhat girlish type of half-timid vanity. His friends had long regarded him as an extraordinary person, possibly a genius, certainly a puzzle. At school he had done well, especially in such writing as he printed in school journals; had won a really skilful control over several forms of verse, had tried his hand at romantic prose with fluency, and had always shown a good deal of artistic sensibility. Mentally he still retained a rich element of true naïveté about

him, despite his maladies. He had an intense though romantically vague love of nature, of living creatures, of young children, of tender and sweet things generally, and this fondness again was often expressed with a relatively feminine enthusiasm and simplicity.

But now, on the basis of this child-like and so far keenly suggestible nature, with its sensitive but physically vigorous *naïveté*, there was superimposed a second nature, colored, and partly determined, apparently, by the inherited bent and the acquired habits of his sexual life. The latter had gradually become a life of excesses and of pronounced and openly defended libertinism. His disorders in this respect were reinforced by considerable capriciously irregular drinking, by many cigarettes, and by much strong coffee. As to all these habits, my charge was absolutely stubborn, had no moments of repentance, never was suggestible, in this region of his life, and occasionally became, if reasoned with upon such topics, strangely brutal in tone, especially in letters which he wrote to me, and which contrasted singularly with his gentleness when in my company, and with the almost uniform suggestibility of his moods whenever we talked together. These incidents of what proved to be a decidedly pathological love of excitement were, however, not the most immediate of the symptoms of mental disturbance. At all events the dissipations were not the mere overflow of a wastefully vigorous physical nature. They were, as it proved, the accompaniments of a highly ominous eccentricity of general mental temperament. My charge had already shown, in writings produced in his later boyhood, and submitted to me amongst the rest, a strong tendency to a partially incoherent wealth of half automatic trains of words, images, and ideas. This trait remained in him during the time when I knew him, and, while it was plainly made worse by his excesses, I could not at all refer its origin to these habits. For the elements of the process were all present in his writings at fifteen years of age, while his physical habits were of recent growth. The trait never showed itself in his speech in any such form as in his writings. His set compositions at school, and in school papers, failed to show his defects.

But they were manifest in all that he wrote for himself. It was when he was alone that the impulse to this half-automatic thinking, imaging, dreaming, and writing would seize him. Then came processes whose character was decidedly marked, and very often repeated. A wholly imaginary scene or situation, usually represented in pretty vivid visual terms, would come to mind, and my charge would begin to weave a story about this scene, or to elaborate the matter in a poem or to write an essay. From the outset this scene or situation would seem to the man himself, however, not the mere beginning of a possible train of voluntary production, but an insistently significant *symbol* of something pretty mysterious, and very vague; and his process of composition was always an effort *to find out what the symbol meant*. The sincerity of this inner attitude towards his symbolic images I had occasion to test in many ways, and I became very sure of the genuineness of my subject's expressions as to this matter. He had, to be sure, as yet, no trace of any system of interpretation, and no actual delusions as to the real existence of any definite kind of wisdom to be gained in this way. But the inner questions: What does this symbol mean? What is this that has come to me? How can I find out what I mean by this idea?—these were at such times simply insistent questions, and they forced upon the subject a perplexing and fascinating sort of brooding, which filled up altogether too much of his life when alone, and, at the time when I knew him, determined a very busy activity of literary composition. The symbols varied very widely from time to time, both as to content and as to kind of significance. Now they were romantic situations, involving forests, ruined castles, mysterious mansions, lonely streams. Now they seemed to be of a more purely metaphysical implication. The result of the appearance of such a symbol might be some hours of silent brooding, or of half-automatic writing, which was carried on with a strong sense of combined delight and puzzle, with a good many marked but capricious changes of bodily sensations—flushings and other physical excitements of various content, which were often carefully noted as the man wrote. The result was never a solution

of the puzzle; on the contrary the tangle was always increased, until the subject abandoned his case in weariness. The most of his actually completed compositions were short poems, seldom or never free from some marked defects of form, but occasionally decidedly skilful, and, in some instances, remarkably coherent, and even, in themselves, promising. Here my subject's wide reading, and his sense for verse forms helped him, although again the influence of Walt Whitman was often disastrous. But the poems never solved his problems. On the other hand his prose remained, at the time when I knew him, always fragmentary. It was devoted to the symbols, and was consequently hopelessly formless, often degenerating in various places with the most frankly avowed incoherence. At such moments the writer would plainly say that he was dealing with the inexpressible, and must simply do what he could. The composition of this prose was dominated by the aforesaid ominous and uncontrollable automatism of associative processes. Images, self-analysis, new puzzles, occasionally new symbols, trooped in masses. The writer could only look on, and report his inspirations. To be sure, he never quite lost track of his original inquiry, and often returned afresh to his starting point, in such a way as to show clearly the insistence of his dominating question. But the story, or essay, or analysis, or confession, to which the symbol gave rise, was a chaos of uselessly recorded broodings, as far beyond rationally definite control as were his often lively dreams when really asleep. Characteristic of the case it was however that the steady sense of wonder and perplexity never left him in all this composition, and this alone gave to his papers any genuine unity, and saved them from being a mere record of a flight of ideas. They had no result; but they always had their precisely defined purpose, viz., to solve the mystery of the meaning of this symbol.

But my subject did not live altogether alone. His dissipations were carried on in company, and this company included many people. And now appeared the other side of his case. His social sensitiveness, influenced, as I judged, by his strongly sensuous nature, was as remarkable as were

his automatic processes. In conversation, I have said, he was kindly and suggestible. His sense of perplexity seldom wholly left him, and often made him converse in a curiously broken and fragmentary way, with some of the confusedness, although never with the automatic wealth, of his writings. But apart from this, his social sensitiveness showed itself in the form of an endless series of somewhat feminine, and seldom ungraceful poses. He assumed various attitudes, expressed various moods, ideals, aims, according as the conversation led him. He himself complained sometimes of an inner sense of insincerity in these poses; while the latter actually had the same kind of automatic insincerity that one notes in the dramatic attitudes of many of those more or less hysterically diposed women, who, when in company, are not merely normally plastic, but are even fatally at the mercy of the now suggested conversational mood or bearing or impersonation. To be sure, my subject, at his worst, never had so wide a range of poses as such an hysterically disposed woman would have, but was constantly limited by his insistent inner wonder as to why he was doing and saying all these things, when probably he meant none of them. Here then was a second source of confusedness in his life. To one who saw as much of bad company as this man, and who also sought out many other kinds of company, this automatic suggestibility was likely to prove almost as disorganizing as were his stubborn lonely broodings.

To complete the picture one has only to note that my subject's social sensitiveness especially showed itself in the form of certain intense and instantaneous impressions which he had concerning people's characters when he first met new acquaintances. These absolutely self-confident seeming intuitions of character phenomena which, as you all doubtless know, are not infrequent as an automatic emotional process in certain sensitive persons, usually took for my subject the characteristic form before described. They were namely, in him, intuitions which appeared as symbols, mysterious, attractive, baffling, like the symbols of his lonely broodings. Only these symbols of characters came to him as reflexes whenever he first met some person who chanced to attract

his notice. At the sight of such a person there at once flashed into his mind the symbol—a scene, a typical mythical act which this person was at once visualized as doing, or again, a wholly mysterious inanimate object, or the inner vision another person, apparently very unlike this one. The symbol came with the feeling: 'This means what, at heart, this new acquaintance truly is.' But meanwhile came also the insistent question: 'What does this symbol mean?' For the symbol was seldom or never one of any sure meaning at all. Only, as my subject told me, whatever he later came to learn of the new acquaintance's character, always got assimilated to the symbol, and served to confirm or to explain it. The symbol thus, of course, never turned out to be inapplicable. But in further intercourse my subject always watched with insistent eagerness for every clue that the new acquaintance gave of his true personality. My subject consequently loved to stare, with a characteristic intentness, at people's faces and movements. This broodingly curious stare he tried, because of his social geniality, to conceal, and further, his frequently puzzled self-absorption combined with other motives to give his facial play and his gestures, when in company, a singularly unequal and inconsistent seeming. Now he looked down long and steadily, with a puzzled smile, at his hands; now he glanced up slyly and timidly as he talked; now giving way to his curiosity about character-study, he stared at you eagerly with an expression of rapt absorption, and again assuming one of the aforesaid dramatic poses, he gave himself over to the momentary mood, and acted more or less completely in character, often adding the observation that he doubted his own sincerity all the while. But of the sincerity of the experiences with the character-symbols there could be no doubt. For some of his lengthiest essays were devoted to character-studies founded upon just such symbols, whose possible meanings he developed in the aforesaid formless fashion. The imagery of the symbols often had, for the rest, a suspiciously coarse and cynical content.

Here, then, on the foregoing theory, were the most manifold materials for abnormal habits of self-consciousness:—a

notably variable common sensibility, heightened by the now moderately irritating results of my subject's toxic and other excesses; a large collection of fascinating automatic associative processes, usually felt to be uncontrollable; an inner stubbornness of self-will, inconsistently linked with an excessive social plasticity, which resulted in many poses, also uncontrollable; a collection of socially determined emotional reflexes, which expressed themselves to consciousness in the form of the character-symbols aforesaid, and which led to an absorbing disposition to brood with an ineffective curiosity over the inner life of other people. All this occurred in a brain of more than average although formless wealth of intellectual processes, and in a man of some artistic taste and sensibility, and of considerable, although decidedly irregular, cultivation.

The actual result was a fairly monumental disorder of self-consciousness, which pervaded the man's whole work and life. That, amongst other things, this man for a while played at studying philosophy, you will perhaps find not surprising; but his philosophical study was of the crudest and most fragmentary sort, and served only to give him a few phrases in which to embody his puzzles; and, for the rest, I warned him away from all such studies, so soon as I had fairly made out his condition. For such men as he was philosophy, as I told him, can indeed do only mischief. But whatever his phrases, it was not any serious philosophical reflection, nor any other theoretical motive, that guided him when he brooded over the endless and insistent problem of problems in his life, viz., the question: "Who am I, and what do I really want or mean in this world?" Since he was fifteen years old, as he repeatedly told me, he had simply been waiting, in growing chaos, in idleness, in dissipation, varied by his activities as a writer—waiting till light should come as to who he was, and what he was here for. With a pathetic eagerness he used to beg me to make out his case, and to answer his question, that he might learn to live, and see his way out of the darkness. But as a fact, since he was emotionally a cheery man, despite all his perplexities and his occasionally keen sufferings, he really did

not want to find any way out at all. His real interest in coming to me was simply to get a listener. He once called his inner world, just as it was, his fairy land. He was plainly minded to stay there—and in the end so far as I was able to follow his career, he stayed. For some years I have lost sight of him. Of course, while he was near me, I did what I could; but the case was too temperamental for any effective treatment.

One example of my subject's style of written work must end this sketch. I choose almost at random, but not for the sake of illustrating what was least sane about my charge. On the contrary it is the slighter variation from the norm which is often most instructive. My records of the case give me such processes by the dozen. And this example is not by any means amongst the worst as to coherence. There is no reason to suppose the following passage to have been written under any direct toxic influence, and what I knew of my subject's habits rendered such an hypothesis, in this instance, quite unnecessary. This was his routine fashion of half-automatic brooding when alone. On this occasion he had been writing for an hour or two, in an essay spontaneously prepared for my eye, concerning a certain ideal that had come to him, after reading Newcomb's Popular Astronomy,—an ideal of an impersonal and heavenly sort of self-possessed wisdom, which, as he just then fancied, he desired to attain. What follows is a description of a warfare between this ideal sort of selfhood, and the passions of his usual sensuously chaotic life:—

"I mean to try to justify myself. Judge you. I'll listen some time when you have fully made up your mind about me. I think I am playing with parts of my character to get rid of them. Do you know I think I haven't any identity at all, down at bottom. I realize it when I am writing in this way. I feel almost mad. I am so out of my ordinary self of personal contact—and squeamish sensitiveness, when touched on occasion to the quick by the living forms about me in intercourse with them. Here's your deepest problem of psychology—the identification with the absolute. I mean the above seriously. I want you to con-

sider it. My sensations on these occasions are extremely peculiar and complex. I feel beyond what I have supposed to be myself, utterly, and yet there lingers the remembrance, and when I stop and head the remembrance, there comes a sharp conflict—an extremely sharp conflict—a mixed feeling in regard to self, as if I were two personalities, two selves—and another self were first turning to one then to the other (and yet it is not the *real* self—and yet again not unreal) and considering which is the right one. When feeling the impersonal self, feeling at the same time that it *must* somehow include in it the personal squeamish self—the one whose desires are gratified—who enjoys existence, the world—eating, drinking, loving, and feeling if it cannot have it, how it must be giving up all the joys of existence—everything that makes life worth living—how if it sacrifices itself it must feel infinitely worse than one feels it has an extremely sore tooth pulled from one's head—how it would be *mad*—insane—being another self than the natural one—yet feeling that the personal self *must* go—that the sore tooth must be extracted once and for all—and yet that it can-*not*—absolutely *cannot* part from it—for then it would be (yes I mean it—this is the sensation) *naught*—or mad—not myself—a mere machine—somehow—that it cannot realize it otherwise, and just so the feeling goes with the personal self in predominance—only then the impersonal self is so vague so far away—except when writing in this way and on several other like absorbed occasions—or in thinking of future self-conduct, etc.

"Well to renew—the fact of renewing brings me back of course nearer to the narrow personal self.—Oh how can I give up that self!—madness—without the joys of existence—nought—machine—not a self at all—for Sir Isaac Newton had a decided self—and so has Professor X.—they're all narrow more or less (and how can I sacrifice myself—this body and brain cannot even hold the enlarged comparative impersonality of Sir Isaac, without madness—being beside one's self—out of one's self—for he was so constructed as to be that comparatively impersonal self. He was—and I am not—I feel it. But time will tell providing the change is gradual

eh?—And meantime I get rid of much of the burden here—unless the associations occur again too strongly).

"My theme has grown. I'll wait to catch the threads and then if possible condense.—Meantime I am hot. My head feels stuffy. I feel almost that impersonal self already (queer phraseology this—"impersonal self—This remark a part of personal self). I feel without usual bodily sensations—a fact—without usual sensations, thoughts—ways of thinking—yet stuffy and warm about head and body.—So I say to myself, I give myself up to you to make what use of it you can. The personal self—the narrowest—cries for recompense—says I am foolish—even in saying this 'foolish' foolish—says I may be ridiculed.—The more impersonal steps in and says, What then the difference? You (that is I) may be foolish but he (you, Professor Royce) makes use of it—and he understands—you wish to be understood—you have no object—not much object even in this—*but* let the writing go to him. What after all the difference?—And he makes use of it—and you express yourself which after all is a good thing—but again for whom?—yourself—myself. What object again?—justice? love? Who feels the love? Love for Professor Royce?—Why he laughs in a personal way—enjoys himself at Symphony Orchestra Concerts—not *altogether* for the absolute and the progress of the race. He perhaps laughs now—Who? Professor Royce—at me—then I'll quit writing,—no again, What is the difference? But if no object once more answer me. Why do I write? After all it must be for self. No—yes—but again what is the difference? For self once more—for love—for the very fact that you are indifferent—no and yes again, etc. So the contest goes on and after all I keep on writing—yes I believe for myself. I believe I'm sure of that."

My theses in the foregoing have been:—

1. Self-conscious functions are all of them, in their primary aspect, social functions, due to the habits of human intercourse. They involve the presentation of some contrast between Ego and non-Ego. This psychological contrast is primarily that between the subject's own conscious act, idea, intent, or other experience, and an experience which is re-

garded by him as representing the state of another's mind. By means of habits gradually acquired, this contrast early comes to be extended to include that between one's inner states and the represented realities which make up the physical world.

2. In the primary cases of contrast between Ego and non-Ego, the former—the Ego—always includes (for reasons which have been explained in the foregoing), the present modifications of the common sensibility, and the feelings of the sense of control, where these are present at all. The latter, the psychological non-Ego, is a colder, a more localized, and less controllable mass of mental contents.

3. Emotional states, and in general all those modifications of the common sensibility which uniformly accompany any of our social reflexes, become, by association, linked with our memories and ideas of social situations, and cannot be repeated without more or less clearly or vaguely reminding us of such social situations in an individual or in a summary form.

4. When social situations involving particular contrasts of Ego and non-Ego are remembered or imagined, we become self-conscious in memory, or in idea. When emotions, associated by old habit with social situations, dimly or summarily suggest such situations, with their accompanying contrast of Ego and non-Ego, our self consciousness gets colored accordingly. Finally, when the varied contents of our isolated consciousness involve in any way, as they pass, contrasts which either remind us of the social contrast between Ego and non-Ego, or excite us to acts involving social habits, such as questioning, or internal speech, we become reflectively self-conscious, even when quite alone with our own states.

5. The anomalies of self-consciousness are (1) primary alterations of the common sensibility, or of the other contents of passing consciousness, such as dimly or clearly suggest anomalous social situations, contrasts and functions; or else they are (2) primary anomalies in one's social habits themselves. The two forms can be of course to any degree combined.

THE PERCEPTION OF TWO POINTS NOT THE SPACE-THRESHOLD.

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In the older psycho-physical conception of Weber and Fechner, the space-threshold of a locality on the skin is that distance of two stimulating points from each other at which they are at first perceived as two. The classical works of Weber, 'De Pulsu, Resorptione, Auditu, et Tactu,' and 'Tastsinn und Gemeingefühl,' first excited physiologists and psychologists to seek an exact knowledge of this distance for different localities on the skin and to form some physiological explanation of its regularities and variations. Fechner, using the terminology of Herbart, first named this distance the *Raumschwelle*, and the term has come to be used to a greater or less extent in psychological literature. The conception is mathematical in so far as it is based on the geometrical fact that two points are necessary to the simplest form of space-extension. It is physiological in so far as based upon Weber's theory of sensory circles, according to which two or more 'sensory circles' must lie unstimulated between two 'touched circles' in order that space, in its simplest form, be tactually perceived. The conception presupposes that there is a space-threshold; that it is the point of transition from the sensation of one point to that of two; and that it is to be found either by the so-called 'method of least perceptible changes' or by that 'of right and wrong cases,' provided the answers collected be passed through one or another of the formulas of Fechner, Müller and Camerer, all of which are based upon the Gaussian formula[1] of the theory of Probability.

These three formulas arose in connection with the method of right and wrong cases which Vierodt first formulated and

[1] This formula contains but two variables.

applied.[1] It was found from the first that between the sensation of one point and that of two, a variety of sensations which can neither be classed as those of one point nor of two appear. Of the pupils of Vierordt in the physiological institute at Tübingen, Kottenkamp and Ulrich[2] divided the sensations which appear in such experiments into the following classes—I. Double sensations, including *a*) those with a correct and *b*) those with an incorrect judgment of the affected spots of skin; II. simple sensations, *c*) pointed or *d*) as if the skin were touched with a long-shaped instrument, *α*) correctly so felt and *β*) incorrectly. Out of these cases they included only I *a*) under the category of 'right judgments,' leaving all the others to the class of 'wrong' ones. Paulus[3] and Riecker[4], as also Schimpf[5] and Hartmann[6] adopted the same classification, adding only the answer 'undecided' to the list of 'wrong cases.' In his first series of experiments,[7] Dr. Camerer subsumed 'all sensations which cannot be nearer described than that they seem to be produced, not by one pin-point, but by something more extensive,' among the cases of 'right judgments.' But in his later series[8] he accepted the four answers, 'two points,' 'more than one point,' 'undetermined,' and 'one simple point.'

To dispose of these troublesome groups of intermediate sensations, the three mathematical formulas of Fechner, Müller, and Camerer, each claiming superiority to the other two, were constructed. Their purpose is to reduce, by a simple calculation in the Theory of Probability, this numerous group of intermediate answers to the two variables, r and f, or r and z, which the formulas contain. In Camerer's first experiments in which the answers were 'one point,' 'two points,' and 'undecided,' that latter group were evenly di-

[1] Unterschieds empfindlichkeit im Schallgebeite—Vierordt's Archiv, 1856, Heft 2, p, 185.

[2] Versuche über den Raumsinn der Haut der oberen Extremitäten, p. 42.

[3] Versuche über den Raumsinn der Haut der oberen Extremitäten, p. 3.

[4] Versuche über den Raumsinn der Kopfhaut, Tabelle II, p. 14.

[5] Raumsinn der unteren Extremität bei Anchylose des Kniegelenks I, p. 11 and ff.

[6] Raumsinn der Haut des Rumpfes und des Halses. Tabelle I, p. 7.

[7] Versuche über den Raumsinn der Haut nach der Methode der r. u. f. Fälle, I.

[8] Versuche über den Raumsinn der Haut nach der Methode der r. u. f. Fälle, No. II, p. 285 ff.

vided between the two classes of 'right' and 'wrong judgments.' That all these methods leave much room for gross inaccuracies in results seems admitted by all. Nor do the elaborate formulas settle the question. The discussion of their relative values seems to have died with their champions, and the applicability of the methods of right and wrong cases to the determination of the so-called *Raumschwelle* is still an open question in the school of Psycho-physics, as the late discussions of Merkel amply demonstrate.

A more recent view has offered a somewhat different conception of space according to which it is based upon a quality of sensation as such. According to Külpe this quality, viz., extensity (*Ausgedehntheit*) belongs to sensations of sight and touch[1]: according to James[2] and Ward,[3] to all sensations. In connection with a series of experiments to determine the effect of exercise on the perception of two points, it was thought that a new side of the facts in regard to the tactual perception of space might be gained by asking the observer to describe his sensations, as fully as possible, giving their spatial characteristics and, in connection with the perception of two points, their apparent distances apart. A large number of the descriptions received are difficult to classify and cannot be conveniently given in the form of tables; but enough can be thrown into the following groups to convince one that every sensation of touch has a space-quality which at once becomes apparent through the comparison of two or more different sensations with each other.

The observers in these experiments were Herr Max Arrer (Ar.), M. Victor Henri (H.), Rev. S. Gringe Hefelbower (Hef.), and Messrs. G. M. Stratton (St.), A. Müller (A. M.), and G. Tawney (T.). We wish here to express our thanks to these five gentlemen for their indispensable assistance. Table I. gives the cases in which one sensation only was felt. In the first vertical column are the observers; in the following four the applied stimuli, viz., one point, two points whose distance apart is below the threshold for the

[1] Grundriss der Psychologie, p. 347, § 3.

[2] Principles of Psychology, Vol. II, Chap. XX, p. 135.

[3] Encyclopædia Britannica, Article 'Psychology,' pp. 49, 53.

perception of two points, two points near the threshold, two points over the threshold; in the following four columns are given the answers received, thrown into the following groups: 'small,' 'sharp' or 'pointed;' 'medium,' 'round' or 'good;' 'large,' 'blunt' or 'extended,' and 'a line' or 'lengthy sensation.' The adjective 'good' was used by nearly all, and when asked what they meant, they answered 'medium-sized,' 'round,' 'solid,' 'not to be mistaken,' 'easy to recognize,' etc. The instrument used in all the experiments was a simple pair of compasses, into which fine, carefully-prepared bone points had been inserted.

TABLE I.—Descriptions of 667 single sensations in terms of space, the stimuli being 1 point, 2 points below the threshold, 2 about the threshold, and 2 over the threshold.

OBSERVER.	STIMULUS.				ANSWER.			
	One point.	Two points under the threshold.	Two points about threshold.	Two points over threshold.	'Small,' 'sharp,' 'pointed.'	'Medium,' 'good' spherical.	'Large,' 'blunt' 'extended.'	'A line' or 'lengthy sensation.'
Ar.	12	84	58	13	2 11	7 30 20 1	1 3 11 3	2 40 27 9
H.	45	27	3		23 6	5 6 1	12 11 2	5 4
St.	26	22	3		7 3	6 3	2 6	11 10 3
A. M.	3	39	30		3 10	2 11	27 19	
Hef.	30	84	77	21	20 7 4	10 17 6 1	8 10 9 2	2 50 58 18
T.	56	22	4	8	15 12	15 2 2 4	17 4 1 3	9 4 1 1

In this table the sensations shift gradually from the first column, 'small and pointed,' toward the last two, as the stimulus passes from one point to two points over the threshold. In the cases of H., A. M., and St., the absence from the table of experiments with two points over the threshold is due to the fact that these observers seldom or never mistook two points over the threshold for one point as the others so often did. The table shows that the space-quality of the sensations of different persons varies widely. Only a very general regularity exists between them. A. M. seemed not to experience single long sensations at all, while St. and Hef. seemed to have more lengthy ones than any other kind. I touched the arm of A. M. with the edge of a visiting-card and asked whether he ever had similar sensations from the compass-points. His answer was an unqualified no.[1]

Table II. gives experiments in which two sensations were felt and described. In the first two vertical columns are the observers and the stimuli for each; in the following seven are the judgments, divided into two classes, where the sensations were alike, and where they were unlike or different. In the first class the two points are alike and either 'small' and 'sharp,' 'medium-sized' and 'spherical,' 'large' and 'blunt,' or 'two points with a line connecting them;' in the second class the points are different: 'the one large and the other small,' 'the one lengthy and the other round,' 'different in space-quality, but connected by a line or long sensation.'

[1] It may be significant that the muscles of H. and A. M., those of A. M. especially, were hard and round at the investigated places, filling out the skin so as to prevent its movement; while those of St. are comparatively soft, and those of Hef. rather fleshy, permitting the compass-points of their own weight to sink into them and thus causing comparatively extensive movements of the skin. This may explain the fact of their frequency with St. and Hef. and their infrequency with H. and A. M. In any case the cause of these variations seems to be chiefly peripheral, as distinct from imagination, expectation, etc.

TABLE II.—Descriptions of 1063 double sensations, 765 alike and 298 unlike, the stimuli being 1 point, 2 points below the threshold, 2 points near the threshold, and 2 points above it.

Observer.	Stimulus.	Two Points Felt Alike.				Two Points Felt Unlike.		
		'Small,' 'sharp,' 'pointed.'	'Medium,' 'spherical,' 'good.'	'Large,' 'blunt,' 'diffuse.'	'A line,' or 'lengthy sensation between.'	'One large, the other small.'	'One lengthy, the other round.'	'Two unlike points connected.'
Ar.	One point.					1		
	2 p'ts under threshold.	10	5	2	1	20	10	5
	2 p'ts about thres.	15	12	5	6	26	16	3
	2 p'ts over thres.	25	20	15	1	2		
St.	One point.	28	25	6	23	8	5	2
	2 p'ts under thres.	31	20	13	30	11	4	4
	2 p'ts about thres.	5	5	6	5	1	1	1
	2 p'ts over thres.	7	9	13	3	2	1	
A. M.	One point.	5	1			2		
	2 p'ts under thres.	3				7		
	2 p'ts about thres.	6	7	4	1	3	4	
	2 p'ts over thres.	5	22	1		7		
H.	One point.	22	22	10	8	1		
	2 p'ts under thres.	7	5	1	2	20	5	10
	2 p'ts about thres.	4	3	1	5	25	3	16
	2 p'ts over thres.	23	19	7	2	2		
Ta.	One point.	50	21	1	27	38	4	6
	2 p'ts under thres.	7	2		6	1		
	2 p'ts about thres.	4	1		2	2		
	2 p'ts over thres.	61	32	4	15	15		5

Table II. gives ample illustration of the fact which we have in hand, viz., that all sensations of touch have a space-quality. It will be noticed that the larger proportion of the cases where the two points are different are stimulated by one point or by two points under the threshold. For example, Ar. felt two points alike 10 times and unlike 20 times when the stimulating points were below the threshold, 15 times alike and 26 times unlike when the points were near the threshold, but 25 times alike and only 2 times unlike when the points were over the threshold. This fact accords

with the self-observation of Ar. that there is always a difference between the two sensations of a so-called *Vexirfehler*, where two points are felt where only one is touched, such that he can in most cases recognize the illusory and the genuine points. But this was not the observation of St. or A. M., but rather the opposite. In the case of Hef. the two sensations from two points over the threshold were always felt as separate, round, solid, and perfectly alike. But what the cause of these differences in different observers may be we are not able to surmise owing to the lack of a large number of observers. The false perceptfon of two points where only one point was touched was most frequent with St. and T.; and least frequent with Hef. who seems to possess in general a very highly developed and very healthy sensory nervous system.

Variations in the 'threshold' were frequent with the same individuals, not only from day to day, but also within the same hour. One observer was found in Wundt's institute who has taken part in numerous skin-experiments, on the volar side of whose lower arm a 'threshold' could not be found which remained constant for a half hour; a similar experience was that with St. and T. Moreover, we made the attempt to repeat the same experiment several times in succession under exactly the same conditions. An example of the results obtained is the following. The place is the volar side of St.'s right lower arm, as it lay unmoved throughout the experiments on the table. The distance apart of the points was 20 mm. The spots on the skin were the same in each experiment, the time interval being always about two minutes. The pressure in each trial was the same, viz., the weight of the compasses. His answers were as follows;

First experiment—two points, 15 mm. apart, clear, equally strong, simultaneously and immediately perceived.

Second experiment—at first a line; then two distinct ends which became perfect points about 30 mm. apart but connected by a line.

Third experiment—one point, sharp, deep, somewhat painful.

Fourth experiment—two points separated about 20 mm., but lying at right angles to the above two points.

Fifth experiment—one point, somewhat large.
Sixth experiment—at first several points: then three became clearer than the remainder: at last one seemed a real point surrounded by a group of fainter ones.
Seventh experiment—at first two points bound together by a line: then a large lengthy sensation about 15 mm. in length.
Eighth experiment—two points about 12 mm. apart, clear, equally strong and simultaneous.
Ninth experiment—one point, small, simple, and definite.
Tenth experiment—two points, 10 mm. apart, simultaneous, equally strong, becoming painful.

Experiments similar to these were made on H. and, later, by H. on T. with the same general results. Such variations are well known to every observer of skin-sensations. The genius of Fechner did not succeed in reducing their manifoldness to simple regularity. Such experiments seem to show clearly that the perception of two points takes place under conditions too varying and too different to be regarded as the first tactual space-perception. Our tactual sense of space seems to be far more exact and far more regular than the perception of two points.

From these and similar experiments it seems that there is no such thing as a 'space-threshold' in the entire field of skin-sensations, because there is no sensation of touch, not even that of a fine needle-point, which does not already possess a spatial quality. The latter does not enter into sensations of touch at the perception of two points. The mathematical point, a point without extension, does not exist either to sight or touch. Geometrical extension in one direction begins with two points, but tactual extensity-perception clearly begins with the comparison of simple tactual sensations. The difference between a point and a line like the edge of a visiting-card is sooner perceived on the lower arm at least, than the difference between two points, thus showing that the perception of extensity through touch does not depend upon the experience of more than one simple sensation. We are fully convinced that the sensation of one point, however fine, has in it the data for abstracting three dimensions by comparison with other points, *i. e.*, by the usual

process of assimilation and discrimination which underlie all perception. The space-threshold should be a certain moment in sensations where extensity, *i. e.*, spatiality, first enters consciousness; but the *Raumschwelle* of Weber and Fechner is the moment where two simultaneous touches enter consciousness which we have seen comes much later and under much more varying conditions—it is in short not a *Raumschwelle* at all. If we wish to speak of a space-threshold at all, we should designate by the term a fact of assimilation rather than any measurements on the surface of the skin. 'The fineness of the locality sense' (*Feinheit des Ortsinnes*) is, properly speaking, the object of all such measurements, but never the 'space-threshold.' We have shown that single sensations and double sensations are both indefinitely various, but the variations are not without some regularity corresponding to the outer stimulus. The single point, the line, the surface, and even the solid, are all perceptions of touch which have their origin in the subjective and objective conditions of the sensations. In short, we have here a large field of sensations which has never been exhaustively investigated. Sensations belonging to this field have, until very lately, been regarded as mere hindrances to the ascertainment of the *Raumschwelle*, and have been either ignored, as in the first experiments of Camerer and those of Vierordt's pupils, or dealt with as food for psychic threshing-machines, such as the formulas of Fechner, Camerer and Müller.

Finally, the conception of a *Raumschwelle* is nothing more than a remnant of the old way, 'von oben nach unten,' of 'Scholastic deduction,' which Fechner strove so faithfully to eradicate from psychology. It is the carrying downward, 'von oben nach unten,' of a physiological and mathematical conception—a reading into sensations of the forms of a highly abstract intellect; whereas the mathematical conception is in fact an abstract of the spatial quality of sensations themselves. It may be that when psychologists have studied sensations humbly and exhaustively they will find among them, and in all of them, the germs of every flower that blows—of both the form and substance of thought, feeling, and will.

DISCUSSION AND REPORTS.

PHYSICAL PAIN.

Professor Strong's article in the July number of this REVIEW entitled 'The Psychology of Pain' must be welcome by all who agree that the determination of a correct psychological theory of pleasure and pain is of importance at this time. As Prof. Strong has noticed my work in this direction perhaps I may be allowed space to note a few points in which I think his argument lacks cogency.

In the first place I am compelled to dissent from his use of the term 'aspect theory' if it is to cover the hypothesis that I defend. I use the expression 'quale theory' to describe the hypothesis I adopt, for the very reason that I wish to place it in opposition to the theory that holds 'that in every actual state of mind we are able to distinguish these two sides, the cognitive and the affective;' this affective side being 'called its feeling tone.' I object to drawing a distinction between a cognitive and an affective side of an experience: I do not believe there could be a feeling of pain without 'any connection with or reference to cognitive states whatever:' to quote Prof. Strong's words.

In my view 'awareness' of pleasure and of pain is brought about by the same general process that brings to us our appreciation of sensations of special qualities, our appreciation of the intensity of those sensations, our appreciation of the reality or unreality of the percepts which are elaborated as a result of these sensations. In other words, I hold that pleasure and pain are cognized, just as much as intensity is cognized, or just as much as reality is cognized.

Furthermore, I hold that if we assume a special 'affective' activity in all experience, which gives us our appreciation of pleasure and pain; then we are bound also to assume a special mind action to account for the recognition of intensity, for the recognition of reality, and in fact for our appreciation of an indefinite number of qualities of experience. I may be allowed, perhaps, to refer the reader to an article published in 'Mind' for April in which I have spoken of this point at some length. It seems to me that a large proportion of the objections which Prof. Strong raises against the concep-

tion of physical pain under the 'aspect theory' fail to present difficulty under the 'quale theory' which I defend, as I shall now attempt to show.

But first of all I must say a word in general in protest against the method that builds up a theory concerning some special mental state, which has been deliberately separated off for theoretical study from some other mental state which is by general acknowledgment closely connected with it; and the setting forth of such a theory without attempt to relate it to the experience connected with the closely-connected state which for mere convenience of study we have temporarily agreed to overlook.

Prof. Strong labors to gain a true conception of the nature of physical pains without allowing the light given by the experience of pleasure to shine upon his path. If it were necessary to emphasize the danger of such study of pain as though it had no relation to pleasure we might refer to the position in which the painstaking Goldscheider has found himself; he having proclaimed the discovery of distinct pain terminals in the skin as the result of just such a specialized study; but having been lately compelled to withdraw his assertions. Had he studied with thoroughness the nature of pleasure he would certainly have been induced to modify at the start his first statements concerning pain, and would have saved other psychologists at least a deal of discussion and annoyance.

In the second place, I must confess that I feel it to be a bold assumption to separate physical pain from displeasure; for it seems clear to me that all displeasures and pains are closely bound together by the fact that they lead to like resultants in our life of thought and expression; furthermore, even in the region of sensation where the distinction between pain and mere displeasure is most marked, it is certain that what is clearly no more than displeasure may often, in connection with increase of intensity, develop into pain, without the occurrence of any observable break in the experience.

But let us for a moment consider, as does Prof. Strong, only those so-called physical pains which, from the theoretical position that I hold, are in fact no more than certain specially vivid portions of the pain part of the general pleasure-pain field.

Concerning the lesson to be learned from neurology, I agree with Prof. Strong that the facts of nerve physiology as we know them do not establish the 'quale theory' which is under discussion, and that they are certainly far from overthrowing it. I cannot help thinking, however, that Prof. Strong has to some degree exaggerated the

difficulties in this direction, and has therefore underestimated the strength of the favorable evidence.

I have suggested that the so-called 'pain sense' in the skin which is produced by cutting or pricking or by some other violent disturbance in the tissues may not improbably be merely a special sensation *which, under the conditions of experiment, is always experienced in painful phase:* that this special sensation in its non-painful phases we may suppose we are unable to separate in analysis, it thus remaining an inseparable part of some of the complex sensations brought out by pressure or by some other dermal irritations of moderate degree. This seems to bring into line with the *quale* theory the fact of which opponents make so much; viz., that cutting and pricking is always painful to the average man in his normal state; nor does the hypothesis seem to me to be a strained one.

That the pain-giving capacities of the tactile or temperature senses are discerned with difficulty is true, but I do not think with Prof. Strong that we are forced by the evidence to admit that the 'affective coloring' of the tactile and temperature sensations 'never amounts to positive pain.' What we are compelled to admit by the evidence to which he refers is that the patient who is anæsthetic (and hence of course analgesic) as to the 'cutting, pricking sensation,' is often rendered analgesic in other directions, without being rendered anæsthetic, in these directions; and, as Wundt has shown, it is very easy to conceive that this latter condition may occur through a reduction of our capacity to be stimulated to the degree necessary to the production of the proper sensations in painful phase.

It is not at all necessary, therefore, to admit that when under normal conditions we perceive a painful burn, the pain and the heat are called forth by separate nerve fibres.

It does not seem to me to be at all clear that sight, hearing, taste and smell are in their nature analgesic, although Foster, Goldscheider and others do so declare.

The sensations from the non-retinal parts of the eye are so intimately connected in consciousness with those arising by stimulation of the rods and cones that determination of this point in connection with sight seems to me to be well nigh impossible.

A similar difficulty arises in connection with stimulation of specific elements of the organ of Corti in the ear.

No one would claim that tastes and smells cannot be intensely disagreeable, I myself should say that they can be distinctly painful, although it is to be agreed that in these cases the limits of the action are so narrow, if we may so speak, that it is difficult to compare

these pains with the pains produced by cutting where the number of nerve terminals that are at one time stimulated to great excess is in all probability very much greater. Moreover, there is every reason to believe that in both nose and mouth excessive stimulation calls out functioning in other organs than those of smell and taste, functioning which we find entirely beyond our control, and which tends to prevent that excessive reaction to the hypernormal stimulus, which is necessary to the production of marked painfulness.

It must not be forgotten, moreover, that very many of our sensations come into clear consciousness only as the result of the summation of many activities which individually are of too small effectiveness to be appreciable in our conscious life at all: this is evident for instance with our normal experience of atmospheric cold and heat, and with the sensations connected with the rubbing of surfaces. It must be remembered also that in many cases we are unable to separate the sensational elements in a pulse of consciousness from their products or resultants of a more complex nature: and this is notably the case with reference to the mental states induced by the stimulation of eye or ear.

It should not surprise us, therefore, that we find it difficult to discriminate pleasure or pain in connection with many sensations which are themselves difficult to isolate.

Turning now to introspective analysis I would say that the difficulties first suggested by Prof. Strong disappear if one hold, as I do in opposition to most of the 'aspect theorists,' that in most cases, indeed, all sensations are accompanied by at least a minimum of pleasure or of pain, but that there are cases where it must be supposed that neither pleasure nor pain exists, but that then at the same time there exists no 'feeling tone' whatever. The notion that in such cases there must be a zero 'feeling tone' is determined by the view that in every actual state of mind we are able to distinguish an affective side over against the cognitive side; and to this view I dissent, as I have said above.

That in cases of extreme pain we usually fail to distinguish the forms of sensibility to which the pain is attached, is a phenomenon of attention that, to my mind, presents no especial difficulty if we conceive of the pain as I do as a particular quality of the presentation of which the sensational differentiation is another quality. It is of great importance to our welfare that our attention should be engrossed by the fact that we are experiencing an extreme pain, and there is no reason whatever, so far as I can see, to be surprised that

it often does so, to the loss of appreciation of the sensational quality that goes with the pain.

Comparison of this experience of attention to pain, with our experience of attention to other qualities of presentation seems to me to confirm this view. The psycho-physicist in experiments made to determine the laws of intensity is surely able to turn his attention solely to the graduation of the intensity, and in so doing he certainly loses appreciation of the specific mental element which is more or less intense. In a more developed form of consciousness we note at times a persistence of attention upon such a quality as the reality of a perceptual presentation; the questioning as to the reality or the non-reality in such cases becoming all absorbing, so that we altogether lose our appreciation of the elements which form the basis of the quality of realness that we are considering.

From Prof. Strong's conclusion "that pain is distinctly the content of certain cutaneous sensations, as blue of certain visual ones," I of course dissent, not only on theoretical grounds but also as the result of introspective evidence as I view it.

But suppose this were all true, I still cannot help thinking it would be an error to class pain (or pleasure) together with states like heat, cold, touch, taste, smell, hearing, sight, which are commonly known as 'sensations,' if for no other reason than this, that heat, cold, touch, taste, smell, hearing, sight, are all determined by the special action of differential terminal organs, *answering to special forms of stimuli in the environment:* hearing, for instance, answers only to vibrations in the air, sight answers only to vibrations in the hypothetical ether.

Whatever may be thought as to the probability of the discovery in the future of terminal organs for physical pain, I think it must be granted that there is no evidence whatever that there exists any special form of environmental stimulus to which physical pain can be the special correspondent; and if there were no reason other than this, it seems to me that it would be a great mistake to place in the well recognized class 'sensation,' a mental state like pain which lacks one of the most marked characteristics of sensation in general.

Physical pains and sensations may properly be designated as forms of sensibility but surely they must be held to be forms of sensibility of different types.

New York. Henry Rutgers Marshall.

A CASE OF SUBJECTIVE PAIN.

I operated upon Mrs. P. for glaucoma, a disease which is characterized by hardness of the eyeball, great pain, and diminution of the power of seeing. Although cocaine was used, the patient experienced considerable discomfort from the speculum and decided pain from the incision in the cornea and the cutting of the iris.

In fact, when the knife was thrust into the anterior chamber, she almost rose to a sitting posture and attempted to grasp my hands. When the iris was cut she rolled the eyeball very far upward and rendered the procedure both difficult to perform and dangerous to the integrity of the crystalline lens. The operation, however, was finished without injury to this structure, and the eye made a successful and fortunate recovery. The evening following the afternoon on which the operation was performed, the patient suffered considerable pain, for which there was no apparent cause. During one of the most painful paroxysms she thought, by chance, of a young girl who was friendly and agreeable to her and of whom she was fond. She said the painful sensations passed away 'like a wave' as soon as the image of the girl was called up. As soon as the agreeable image passed away the pain returned. Being of an investigating mind, the patient proceeded to call up in succession the images of persons agreeable and of those disagreeable. The agreeable images invariably caused relief and the disagreeable ones produced a 'wave of pain.'

She said that the pain produced by the disagreeable images was greater than that which followed upon the disappearance of the pleasant images. The relief caused by the image of her agreeable little friend was so much greater than that produced by any other agreeable image, that she schooled herself to keep this image constantly before her until she fell asleep through the influence of an anodyne; on the following day the phenomena had disappeared. It is worthy of remark that the other eye of this patient had been operated upon one year before, and although the pain after the operation was greater and the recovery more prolonged, she had no experience as that related. It is also well to state that the performance of the first operation was attended by less pain than the second.

The patient is a highly intelligent person of sixty odd years, sensible and practical, albeit a little given to the use of extravagant and poetic expressions.

The case is unique in my experience, and that it is curious and interesting I think may be predicated with certainty.

NEW YORK POLYCLINIC. J. HERBERT CLAIBORNE.

PSYCHOLOGICAL LITERATURE.

The Foundations of Belief. A. J. Balfour. New York, Longmans, Green, & Co., 1895. Pp. 366.

This book considers beliefs, or certain important classes of them, from three points of view: from the point of view of their practical necessity; from that of their philosophic proof; and from that of their scientific origin. Part I, consisting of four chapters, deals with the first topic under the title, 'Some Consequences of Belief;' part II, also of four chapters, considers the second topic under the title, 'Some Reasons for Belief;' part III, consisting of two chapters, treats of the third under the heading, 'Some Causes of Belief.' The work closes with a fourth part entitled 'Some Suggestions towards a Provisional Philosophy.'

One cannot read the suggestive and often acute discussions of the book without the feeling that the cause of English conservative politics has robbed British philosophy, in the person of Mr. Balfour, of a man whose name might have had an honorable place in the list of British thinkers. As it is, the production of such a work by one of the active political leaders of the Conservative Party, is a most interesting phenomenon. The book more than sustains the expectations raised by the author's earlier work, 'A Defence of Philosophic Doubt.'

The book is stronger in its critical than in its constructive portions, and often reminds one of the brilliant criticisms of the late Prof. Caro of Paris. In saying that the work is especially strong on the critical side, we do not wish to imply that the criticisms are at all captious. The tone of the author is uniformly fair, and his treatment of views with which he differs, even generous. The discussions are more philosophical than psychological, and viewed from the philosophical standpoint the work may be characterized as a critical examination of, and an attack on, the rather shallow empiricism (to which the author gives the name 'Naturalism'), which has reigned for so long a time in Great Britain. It is an attack too which Naturalism can hardly afford to ignore.

The book contains considerable matter of interest to psychologists. Mr. Balfour's remarks on determinism, on 'experience,' on the relation of belief to language, on the relation of belief to reality and on the so-called 'immediate judgments of the senses,' and on numerous other topics are worthy of the psychologist's notice. His discussion of the non-rational causes of belief as distinguished from the rational grounds for belief, and of the influence of psychological 'climates' on belief (pp. 214 f.), are especially good. These are distinctions which seem natural, and even obvious, and yet they are very often overlooked. Inner assent, or belief, as the author points out, is produced in countless cases by custom, education, public opinion, the contagious convictions of countrymen, family, party, or church. "But a small number, at least of the most important and fundamental beliefs, are held by persons who could give reasons for them, and of this small number only an inconsiderable fraction are held *in consequence of the reasons* by which they are nominally supported." "*Mere* early training, paternal authority, or public opinion, are causes of belief before they are reasons; they continue to act as non-rational causes after they become reasons." Looked at from the outside, as one among the complex conditions which produce belief, reason appears relatively insignificant and ineffectual; looked at from the inside, it claims by an inalienable title to be supreme. These are distinctions, we repeat, which have not, perhaps, received the consideration which they deserve in the treatment of the psychology of belief. Mr. Balfour in his discussion of psychological 'climates' and their relation to the rational grounds for belief, does not give as much weight as we think he should give to the *inner* state of mind to which Prof. Huxley alludes in his remark that "belief is the product of two factors, the first is the *state of the mind* to which the evidence in favor of that belief is presented; and the second is the logical cogency of the evidence itself;" but the discussion is, nevertheless, one which will repay reading by those interested in the psychological and epistemological aspects of belief.

YALE UNIVERSITY. GEO. M. DUNCAN.

Dolore e Piacere, Storia naturale dei sentimenti. GIUSEPPI SERGI. Milano, Dumolard, 1894. 12°, pp. 395.

Professor Sergi's book has for its frontispiece Edinger's schematic diagram of the nuclei of the medulla oblongata and pons, and its author's cardinal idea is that this region constitutes a great centre for pleasure, pain, and emotional excitement generally. There are other secondary theses, and the whole is preceded by certain psychogenetic theories to which I will refer first.

The primordial property of all living matter may be called irritability or reagibility. In its simplest state this is merely *trophic*, but as tissues differentiate and combine into systems, the irritability of the muscular system becomes *contractility*, and that of the nervous system *sensibility*. Sensibility is at first *unconscious;* but at a certain stage of complication *conscious* sensibility arises. This latter is nothing essentially new, but only a 'transformation' of the one primordial irritability, of which, however, a large proportion remains 'untransformed' even to the end—even in the highest animals and man. Prof. Sergi proves the essential identity of all the grades of irritability by experiments with anæsthetics. These narcotize and paralyze the motor reactions of the simplest animalcules as well as the highest consciousness of man. The reader will note the monistic point of view ('transformation'), and the divergence from the assumption, so popular just now, of psycho-physiological 'parallelism.'

Unconscious sensibility becomes then conscious sensation, and sensation and movement, being both transformations of irritability, are inversely related—the more movement the less immediate sensation, as we may observe in infants and women (61–2). When the outer stimuli are normal in amount the sensations are 'specific,' objective, and cerebrally localized. They are *painful* when the stimuli are excessive. [What appear to be 'pains of inaction,' or want, are really pains either of excessive *tension* in the unused muscles or of abnormal *irritation* by altered blood, as in thirst, etc.] Excessive stimuli, at the same time that they produce pain, alter the heart-action and the breathing, even in animals without hemispheres. Seeing, then, that pain and disturbance of these vital functions vary concomitantly, Prof. Sergi concludes that they are both functions of the same region, that of the *calamus scriptorius*. "The cerebrum has no other action in pain (or pleasure) than that of *rendering the phenomenon conscious* [this is not explained]: only by this does the latter gain an intellective character, and also because some pains are percepts, being localized" (73). That the brain is not the immediate organ of pain, is also proved by the painlessness of wounds there, an insensibility in striking contrast with the extreme distress caused by sensible excitement of the heart and respiration. Pleasure comes from liberation from pain or want and quickly passes into indifference, as in the deviation of the needle when the electric current stops. Positive pleasures of stimulation (as at a banquet) also exist, and carry diffused organic effects, and are also referred by our author to the bulbar region.

Emotions result from outer stimuli exciting first the cortex, then consecutively the bulbar region, and consequently the circulatory and respiratory organs. Psychic emotion and bodily pain and pleasure thus have the same vital centre for their organ, which is played on from below by physical stimuli and from above by psychical activities. In other words, emotion is *cortically initiated* pleasure or pain. James and Lange are wrong in not finally referring it to the bulbar centre, which is the primordial emotional centre; but the special character of the various emotions is determined by subaltern centres, little psychic organisms, determined by the association of race-experiences, which always coöperate with the primary centre and impress on fear, anger, etc., their special and distinctive characters. The chief of these 'instinctive' centres are that for individual preservation, and the sexual, the parental and the social centres. [Prof. Sergi doesn't make it clear whether these secondary centres are cortical or infra-cortical. Neither does he say distinctly which physiological phase of the process he believes the emotional *consciousness* to be attached to, whether directly to the bulbar discharges or (as in the Lange-James theory) to the motor effects of the same. Pretty surely the latter, as he speaks elsewhere (p. 105) of the hemispheres as 'means of consciousness' of the more vital phenomena. Cf. also pp. 129, 131, 140, etc.] The difference between depressive and expansive emotions is explained by *inertia* or *reaction* on the part of the centres. One and the same objective stimulus, according to its own strength or the temporary state of the nervous system, may simply shock the latter into a state of paralysis (producing, *e. g.*, *fear*) or rouse it into resistance (producing, *e. g.*, *anger*). Much is made of these two opposite kinds of effect.

The genesis of emotions is teleological. Like pleasure and pain they are (within limits) signs of benefit or harm, and lead to actions of preservation or defense. This is explained at length in one chapter, and a new classification of emotions, based on their characters of transiency and permanence, inertia and reaction, is set forth in another. Other chapters contain descriptions of special emotions, their variations and exciting conditions, normal and pathological; and finally we have seven chapters on the *Sentiments*, æsthetic and religious. The latter has been a sometimes useful pathological variation in human history. The æsthetic sentiments do not coexist with useful activities, but, as Spencer says, with activities that are symbolic, superfluous or playful. This, however, is compatible with an originally serious use for functions that now are purely æsthetic. Love-songs and war-dances are an example. The *pleasure*

in all cases is mediated by the effect on the bulbar centre, as the author shows in detail for the case of music, the hearing of which alters pulse and breathing. There are in these chapters a number of fine observations of detail and descriptions of æsthetically impressive situations that show well the beauty of the Italian language. But, on the whole, the author's treatment is superficial, and the complexity of the æsthetic life hardly comes out in his pages. The same criticism may be made of his entire book. It would have been 'epoch-making' thirty years or more ago; but after so much speculation of the same sort has been printed, the reader has a right to statements that are less general and vague. In fact the only thing I can think of as a new fact deduced by Prof. Sergi is contained in his observation that, since bodily and spiritual sensibility both have the medulla for their seat, it is impossible that the same person should be highly sensitive in one way and insensible in the other.

W. J.

Logik der Geisteswissenschaften. Zweite Abtheilung des zweiten Bandes der *Logik.* Zweite umgearbeitete Auflage. W. WUNDT. Stuttgart, Enke, 1895. Pp. 643. M. 15.

In the first edition (1883) the methods of the philosophical sciences were included as a part of the second volume. The enlargement, which has resulted in a separate volume on this subject in the present edition, is due in part to additions in the discussion of Jurisprudence, and to a new extended section on sociology. Much more important, however, are the elaborations in the treatment of the fundamental principles relating to all these sciences, and of psychology, regarded as 'the most general philosophical science, and at the same time the indispensable groundwork of all the rest.'

A full description is given of the two methods peculiar to these sciences, namely, interpretation or the explanation of a phenomenon through a psychical motive, and criticism or the determination of 'values;' together with an account of the special modifications of the more general methods which belong to the natural sciences as well. The principles, namely, analogy with our own experience, influence of social and influence of physical environment, which we make use of in the study of all physical, historical and social phenomena, are discussed at length. It is maintained that empirical laws, in the same sense in which this term is used in the natural sciences, are to be found in the philosophical sciences; but exceptions to these laws occur not only through the operation of other general laws, but also through individual acts, which are, indeed, in accordance with psychical laws, but not general in their appearance. The

only causal laws in the philosophical sciences are the psychical laws of individual experience. It is this fact which makes Individual Psychology the foundation science on which all the rest must ultimately base their explanations. The treatment of the Logic of Psychology forms, therefore, an important part of the general system; and is, further, of peculiar interest as a thoroughly revised and in many points new exposition of the author's views in this field where his influence is of such prime importance.

The general position is the inevitable logical consequence of the well-known doctrine of parallelism. Since the psychical world is in no way causally connected with the physical, but an independent unity in itself, it is evident that a science dealing with psychical phenomena can make no immediate use of physical facts, but must formulate its laws and even its methods on an exclusively psychical basis. This position is emphasized by a vigorous attack upon what the author calls psycho-physical materialism, or the definition of psychology which makes it the mission of this science to investigate the physical and physiological conditions of psychical phenomena. Still more is the standpoint made clear by a complete revision of the description and even the nomenclature of the experimental methods. An example will serve to illustrate. Instead of the common names '*Fehlermethoden*' and '*rechte und falsche Fälle*,' we have '*Abzählungsmethoden*' and '*positive und negative Fälle.*' The reason for this change is that the old names imply that the psychical process is being directly measured by the physical stimulus, and is 'right' or 'wrong' in so far as it agrees with the stimulus or not. This implication is, however, based upon the false notion that a causal relation exists between the physical and psychical worlds. In reality no such relation can be assumed; and psychical processes can be compared only among themselves, measurement being possible in the sense of such exclusively psychical comparisons alone. The whole value and significance of the stimulus is that we employ it as a means to produce the parallel psychical process at a time and under circumstances favorable for comparison with other psychical processes and for more exact observation. The importance of the physical agent as such a means is not to be lost sight of, for it is the only way in which self-observation, in any true scientific sense, can be made possible. But, on the other hand, it is not to be forgotten, as is so often the case, that the means is not the end. In other words, the study of the physical and physiological means is merely auxiliary and preparative, not the final science. Psycho-physics and physiological psychology are to be regarded as transitional stages, not as the real science of psychology.

The author's position may be further briefly characterized as 'Voluntarism.' By means of logical abstraction we divide our complex psychical experiences into elements, among which the one kind that we call volition may be said to be typical. Not that we are to neglect the other elements or attempt to reduce them to cases of volition, but we are to think of all psychical experiences as unitary processes, like volition. It is to be noted that the word processes must be emphasized as much as unitary if we will truly describe the transitory character of psychical experience.

Finally, the name 'Actualism' will serve to define the author's view of the nature of the soul as opposed to 'Substantialism.'

LEIPZIG. CHAS. H. JUDD.

Thinking, Feeling, Doing. E. W. SCRIPTURE. Meadville, Flood & Vincent, 1895. Pp. XII + 304.

This book leaves still unanswered the question whether experimental psychology can at the present time be popularized in a useful and dignified manner. It demonstrates beyond the shadow of a doubt, however, its author's entire unfitness for such a task. Few things have been neglected to make the book bad, and many a reader who, like the present writer, had looked to Dr. Scripture for something substantial and withal creditable to American scholarship, will turn away honestly and thoroughly disappointed. The few redeeming features of the book are found in the generally good typographical work, the ingenuity of some of the methods described, the accuracy of the facts cited, and the clear, if not elegant, style. For the last two characteristics Dr. Scripture is not wholly responsible, as will appear below.

The title of the book indicates something of the nature of the contents, but very little of the method or order of presentation. The opening chapter, on observation and experiment, is followed by six chapters on reaction-times and the peculiarities of will and attention as revealed by experimentation. Then follow seven chapters upon sense perceptions, one each upon feeling, emotion, memory, rhythmic action and suggestion in the order named, concluding with two chapters upon the general subject of psychological standpoint and method, of which the one is entitled materialism and spiritualism, the other the new psychology.

Dr. Scripture may be possessed of a deep and well-conceived method in the arrangement of his material; but, if so, the fact does not intrude itself upon the reader's attention. The chapters read almost equally well in either direction, and one's logical instincts are somewhat baffled by the fact. A fatal weakness of the book lies

just here. There is no vital principle within it. *Disjecta membra* are all that the eager public, for whom we are given to understand the book is expressly written, is privileged to see. These are presented, however, with all the graphic prodigality of a popular monthly. Two hundred and ten cuts to two hundred and ninety-five pages of text is all that the most extravagant pictorial appetite could ask. The author will probably insist, with his usual—and admirable—reverence for *fact*, that his book possesses all the vital unity consistent with the present stock of experimentally verified data. If this be really the case, the work might better have been postponed for a few years. But, in the opinion of the reviewer, Dr. Scripture's lack of system is quite as largely due to his lack of a deep and sound conception of what really constitutes a fact and wherein lies its value. He has become psychologically myopic. This opinion is confirmed by a certain cocksureness of tone characterizing the book, which must arouse the suspicions of even the most unsophisticated reader. Among the more knowing these suspicions will be at once awakened upon reading in the preface that "this is the first book on the new, or experimental, psychology written in the English language." Dr. Scripture may possess sufficient exegetical agility to harmonize these words with the facts; but, with Sanford's manual already in the field, his statement is at least misleading.

In a work largely devoted to expounding the value of accuracy, it is most unfortunate to find such gross carelessness in the use of quotation marks as Dr. Scripture's book reveals. A mere statement of the facts will suffice. If the author has any explanation to offer, common fairness demands a suspension of judgment until he has been heard. If he has not, comment on the part of the reviewer is quite superfluous. Summarized the case stands thus. In Chapter XXI, Dr. Scripture has occasion to quote from Wundt's 'Human and Animal Psychology.' In these passages the English varies widely from that of the Creighton and Titchener translation of this work.[1] But throughout nearly the whole of Chapter XVII, where the author vaguely states that he 'follows' Wundt, the English is not only a translation of the German, but is, furthermore, identical with that of the published translation just mentioned, save for a few rare cases in which synonyms have been employed and the order slightly changed.[2] In the same chapter, even before the expression

The parallel passages are herewith given:

Thinking, Feeling, and Doing.	C. & T. trans. Wundt's Human and Animal Psychology.
[1] Pages 276 ff.	Pages 2 ff.
[2] Pages 226–38.	Pages 372–80.

of intention to follow Wundt, occurs a sentence which is identical with one in the Creighton and Titchener translation.[3] In another passage, a page in length, in which occurs no least suggestion of indebtedness of any kind, the language is absolutely identical with a portion of the Creighton and Titchener text.[4] After completing the quotation cited in Note ([1]) on last page, he continues for several pages to follow Wundt, in a way which is much more nearly translation than paraphrase, and this with no intimation of obligation.[5]

The whole book, and especially the concluding chapter, is essentially a panegyric upon the 'new psychology,' by which the author means experimental psychology of the laboratory kind. In his contempt for what he is pleased to call 'arm-chair psychology,' Dr. Scripture is no more virulent than are such writers as Dr. Ward in their denunciations of Dr. Scripture's school. But there are surely many of us who feel that both parties represent a somewhat obstinate extremism, and that there is a very vigorous, hopeful movement which may properly claim for its endeavors the fortunately vague title of the new psychology. This movement, if it manifests no sympathy for the acrid strictures of Dr. Ward, is equally guiltless of any such cramped and one-sided view as that appearing in the words, if not the meaning, of Dr. Scripture's book. Despite their many shortcomings, the psychological works which the American press alone has produced in the last few years bear ample evidence to the existence of a thoroughly sound empirical spirit, which is ready to accept all facts that can present unquestionably trustworthy credentials, whether from Dr. Scripture's laboratory or his colleague's arm-chair, which has an eye to the vast extent of consciousness throughout the organic universe, its abnormal or unusual manifestations as well as its commoner forms, and which is not afraid to fail in framing and testing hypotheses for such facts as are already at hand; convinced that to search for facts without at the same time searching for their implications, is a shade less rational than the celebrated expedition after the Snark. Were Dr. Scripture's extremism to become legal tender, psychology would be well-nigh bankrupt, and his book is, perhaps, the best proof of this. In it we are, indeed, confronted by a "string of raw facts." We can afford to be grateful for everything the laboratories give us, and we may be as

Thinking, Feeling, and Doing,	C. & T. trans. Wundt's Human and Animal Psychology.
[3] Page 226.	Page 372.
[4] Pages 272–3	Pages 267–8.
[5] Pages 278–81	Pages 6–7.

optimistic as we please in hoping for their future, but to convey to the public the impression that all other work is balderdash, and that we have anything but the crude beginnings of a science, is to lead the blind into a pit; and against this the reviewer, as an advocate of the laboratories, must heartily protest.

UNIVERSITY OF CHICAGO. JAMES ROWLAND ANGELL.

Die Umwälzung der Wahrnehmungshypothesen durch die mechanische Methode. Nebst einem Beitrag über die Grenzen der physiologischen Psychologie. H. SCHWARZ. Leipzig, Duncker & Humblot, 1895. Pp. xx + 198, ii + 213.

Dr. Schwarz writes from the realistic point of view, and frankly avows his obligations to Uphues and Twardowski. Indeed, he goes so far as to say, in a note to page 93, that 'Die richtige Erkenntniss in der Erkenntnissfrage' will not be widespread until the views of these two philosophers have been generally accepted. Yet the historical analysis which he has here undertaken of the conditions under which the old naïve belief in the objective reality of colors and sounds was given up, is on the whole impartial, and will be read with profit by those who have not yet accepted the only true solution of the epistemological problem as propounded by Uphues, Twardowski and Schwarz.

The first section, entitled 'Das Problem des unmittelbaren Erkennens,' exhibits the displacement of the older scholastic theories as to the means whereby the outer object affects the cognizant subject, by the mechanical theories of Hobbes and Descartes. As representatives of scholasticism, Dr. Schwarz selects Suarez's theory of intermediate species, conceived as qualities in flight from object to subject; Thomas Aquinas' doctrine of a phantasm or 'fictum' evolved by the subject out of its own substance in the form of the perceived object; and Gabriel Biel's nominalistic repudiation of both in favor of an unexplained action from a distance by the object upon the subject. Hobbes originally held the still older view of Democritus, that material objects are perceived by means of material particles which they constantly throw off into space, and he attacked the species theory from that standpoint. Later he became acquainted with Galileo's doctrines and adopted a conception analogous to that now prevalent: that the outer object works upon the sense-organ by means of some form of motion. Descartes, a mathematician from the outset, was led to a similar conclusion by the desire to bring all departments of knowledge within the domain of mathematics; and this he saw could be done in the realm of psychology only by ex-

pressing all mental phenomena in terms of motion. Yet in both Hobbes and Descartes we find traces of older conceptions. To Hobbes' mind the modes of motion, whereby the outer object is made known to the subject, are possessed of the same mysterious representative function which the scholastics ascribed to the intermediate species: in some way the mental states they beget in the subject are to be conceived as copies of the outer object. Descartes practically holds to the scholastic doctrine of the 'fictum,' in that he denies of the impressions of sense all true reality; and he also holds Suarez's doctrine, that the real object can be directly cognized by an intellectual 'hapsis.'

The second section, entitled 'Das Problem der Sinnesqualitäten,' seeks to unravel the motives that led Hobbes and Descartes to deny the objective existence of colors and sounds. Hobbes can find in them no reality whatever, unless it be the reality that attaches to them as modes of brain motion; and this is the natural outcome of the attempt to exalt the mechanical method, the true object of which is to bring order into the flux of sensation by means of mathematical concepts, into a metaphysic. According to Descartes, sensations are accidents of the composite of body and soul which we call man, and their function is to guard it against dangers. Descartes' tendency to deny true reality of sensations and to allow them but an obscure and dim mode of existence, is traceable to the fact that sensations are incapable of being wrought into mathematical calculations; and this, to a mind of Descartes' type, was sufficient ground for ignoring them.

It is worthy of note that the mechanical method of modern science is not based alone upon the preference we accord touch sensations—from which our wave conceptions are for the most part drawn—but upon the fact that, by means of these conceptions, we can make the phenomena of color and sound intelligible and predictible. Otherwise it would remain conceivable that sciences of acoustics and optics might be developed which would deal with colors and sounds directly, without the intervention of such auxiliary conceptions. As it is, however, these sciences are sciences, not because they have been reduced to a *touch* basis, but because they have been reduced to a *mathematical* basis; and as no other mathematical basis can be found for them, we may safely assume that our present mechanical method is the final method of optics and acoustics.

The 'Beitrag' is entirely independent of the balance of the volume and is practically a critique of Exner's 'Entwurf zu einer physiologischen Erklärung der psychischen Erscheinungen,' which the

author is inclined to regard as the capstone of the great scientific structure begun by Galileo, Hobbes and Descartes . . . "It devolves, then, upon the philosopher and the psychologist to see to it that his science, in so far as it assumes the validity of other than mechanical conceptions, is not crushed, suffocated, by this crowning development of the scientific Weltauffassung."

The main force of Dr. Schwarz's criticism is directed against two of Exner's fundamental assumptions. If we grant, he argues, that cortical processes are always essentially the same in character, and are differentiated from one another only in intensity and locality, it is conceivable that they form the basis of affective consciousness, since it is capable of variation in two modes only—quality and intensity; but it is not possible to conceive them as the basis of sensations, since sensations are possessed of a third characteristic, outness or position in space, and for this there would remain no physiological expression. The objection cannot be evaded by regarding position in space as a mode of sensations. Furthermore, since sensation and feeling are themselves distinct series of conscious events, the physiological terms in which the one finds expression will not serve for the other. The attempt to reduce feeling to muscle sensation must be regarded as a failure.

Again, if we assume with Exner that coalescence of conscious states is due to coalescence of their cortical processes, we fail to understand the actual diversity of consciousness. We should expect that all coexistent cortical processes would coalesce with one another and consciousness would exhibit a constant progression from one total state to another, whereas, in fact, we find infinite diversity.

Univ. of Pennsylvania. W. R. Newbold.

Monism as Connecting Religion and Science. Ernst Haeckel. Eng. trans. by J. Gilchrist. New York, Macmillan, 1894. Pp. 117.

This address, delivered by the author at Altenburg on the seventy-fifth anniversary of the 'Naturforschende Gesellschaft des Oesterlandes,' sets forth a naturalistic Pantheism. It states in clear terms a monistic theory of the universe construed in a naturalistic manner. By Monism is meant "that there lives one spirit in all things, and that the whole cognizable world is constituted and has been developed in accordance with one common fundamental law." The author lays emphasis on the essential unity of inorganic and organic nature; the latter, he claims, having developed from the former at a comparatively late period, there being no absolute distinction between them any more than between animal and man. The author

then speaks of human knowledge, saying: "Similarly we regard the whole of human knowledge as a structural unity; in this sphere we refuse to accept the distinction usually drawn between the natural and the spiritual. The latter is only a part of the former (or *vice versa*); both are one." Haeckel seeks to prove this by arguments from both the subjective and objective standpoints. From the former he cites the evolution of knowledge in the human race, and mentions what he terms the advance from Anthropomorphism to Monism. From the latter or objective standpoint he uses as his proof the law of the conservation of energy and matter. Uniting energy and matter, he starts with 'animated atoms.' In this connection the author goes on to show the results of the evolution theory in the development of the organic from the inorganic, and consciousness from the former. "Immortality, in a scientific sense, is conservation of substance, therefore the same as conservation of energy as defined by physics or conservation of matter as defined by chemistry." God, he says, can be represented as 'the infinite sum of all natural forces.' Hæckel expects to be accused of materialism, but claims that this is a mere 'party word,' and that spiritualism would describe his theory quite as well. The address is Spinozism in the garb of modern science.

PRINCETON. C. W. HODGE.

GENERAL.

Bemerkungen zum Begriff des Gegenstandes der Psychologie. R. AVENARIUS. Viertelj. f. wiss. Phil. Hefte II, IV, 1894; I, II, 1895.

The writer has undertaken through an analysis of experience in its most comprehensive sense to determine the essential character, relations, and extent of that special form of experience which we call psychical. As soon as we begin to reflect we find that our experience consists of two equally immediate and indispensable elements, an *ego*—including all that belongs to the so-called me, as thoughts, emotions and desires—in the midst of an *environment*—the complex commonly known in philosophy as the non-ego. These two elements can never be thought of as appearing separately; every actual or 'complete' experience must contain both.

Before proceeding to further analysis, it is important to show the fallacy contained in the division of experience into 'inner' and 'outer.' It is evident that this distinction does not exist in the immediate individual experience, but is derived from an interpretation of the experience of others. Among the constituents of my environment are beings who behave and express themselves as I do;

seek certain ends and avoid others as I do; in short, appear to hold exactly the same relation to their environment that I do to mine. I, therefore, conclude that they are in reality just such beings as I am. This conclusion is, however, nothing more than an hypothesis based on analogy, for the acts of my fellowmen must always remain, in so far as my immediate experience is concerned, mechanical processes like all other movements in my environment. And it is clear that, since analogy is my only ground for attributing to these acts a more than mechanical significance, in other words, for assuming that they belong to experiences like my own, I am not justified in introducing into my interpretation of these experiences any content or process that I do not find immediately present in my own. This, however, is just what is done, when instead of recognizing an ego and a non-ego as equally essential elements of another's experience, I 'intraject' the non-ego element into the ego and regard it as an 'inner' process or modification which I call a perception.

A complete experience in the sense above defined exists in reality only as an inseparable unity. Logically, on the other hand, we may divide it by abstracting from one of the elements or parts of the elements. In this case we have 'partial experiences' which, it is to be emphasized, are not realities, distinct from each other and from the complete experience, but logical fictions, or various ways of regarding one and the same experience. What we call physical experience, for example, is not a special kind of experience in the dualistic sense, but a logical abstraction in which we have agreed, for the purpose in hand, to neglect the ego element. The question now takes the form, what partial experiences belong to Psychology? To answer briefly, those which stand in a relation of dependence upon the ego and are in a logical sense determined by this dependence. For example, a stone may be a partial experience in the sense that we abstract from its relations to other elements of the environment and from its relation to the ego. It is then a subject of Geology or Physics but not of Psychology. If, however, we think of the same stone as an element of an experience which we regard in the light of its dependence on the ego, as in a case of memory, then we have a partial experience belonging to Psychology. In short, any experience may belong to Psychology, if the way in which we look at it is in its relation of dependence upon the ego. We may go further. The condition of this dependence is a modification in a central nervous system; hence we may substitute for ego, central nervous system, and our definition becomes 'the subject of Psychology is experience in general in so far as it is regarded as dependent on a central

nervous system.' Not only is it difficult, however, to determine the limits which are set to the actual existence of such central nervous systems; but when we extend the inquiry beyond actual to potential existence, we shall find that we are entirely unable to fix the point where we can say, the possibility of the evolution of such a system is here forever excluded. The question of the extent of psychical experience leads us beyond the limits of our special science.

LEIPZIG. CHARLES H. JUDD.

Der Begriff der Seele in der empirischen Psychologie. JOSEF SCHUCHTER. Brixen, 1895. Pp. 39.

The author makes the usual distinction between rational and empirical psychology, claiming, however, that after empirical psychology has studied conscious phenomena, it can study the nature of the soul on the basis of her empirical facts and laws, keeping to the empirical method. He claims that most authors agree as to this conception of empirical psychology, but some would exclude all questions as to the soul. To this the author objects, as psychology, he claims, would thus lose its pedagogic value and worth as a propædeutic to metaphysical questions. The motive for his work is the questions: "Has empirical psychology grounds for taking the concept of the soul into its domain?" And: "How can this be done in harmony with its method?" Though these questions were the motive for the work, the author distinctly states that they have not affected its form, which, rather than being an answer to these questions, is an attempt to reach a concept of the soul, and to show that empirical psychology furnishes material for the solution of the question; an attempt in which the author frequently leaves the sphere of experience altogether. First the author takes up consciousness, and after describing it says that it is the business of empirical psychology to seek to define it. An analytic definition being impossible, because there is no more ultimate or simple term by which to define it, a 'synthetic' or 'genetic' definition is what is to be sought. This, he claims, is to inquire after the conditions and existence of consciousness; and this, he claims, is equivalent to the problem of the soul. So empirical psychology leads to this problem, and can also give 'hints' as to its solution. The author proceeds to show these, going on with the study of consciousness. Consciousness is the universal form which accompanies mental phenomena. The form has been identified with the content and lost in it. We cannot limit consciousness to this interpretation. When we regard it as something which can in thought be abstracted from its

content, we may ask, what is the abstracting subject? Consciousness thus divides into subject and object, and this same analytic activity is the same as the synthetic activity which binds together conscious phenomena. This is the soul, which is, therefore, an activity. The author discusses the question of the relation of soul and body, and concludes that the concept of the unconscious, being a positive one, mediates between soul and body. He holds Aristotle's idea that the soul is the entelechy of the body, and to be conceived as a potence developing into actuality. But as what has more and richer content cannot be developed from what has less, the idea of God as the source of all things, is to be presupposed.

The work is concluded with a discussion of the question of immortality, into which, for want of space, I cannot go. His argument is from the development of the soul in self-dependence and from certain ideas, as, for example, the good. Because these ideas have a psychological side, the author claims all that for empirical psychology, not making a distinction between their psychologic existence in consciousness and the question of their meaning, which is metaphysical. Thus it can be seen, all along the discussion, that he has not kept within the sphere of empirical psychology.

The Integration of Mind. EDMUND MONTGOMERY. Mind, Vol. 4, No. 15. Pp. 307–319.

What we perceive has only momentary existence. Whatever made up the content of consciousness, the preceding moment has forever vanished out of existence, and the following moment is as yet non-existent. That which seems to endure in identity can never be that which is consciously present to us. Even could we obtain a 'punctum stans,' there would still arise the question as to the hidden source under the totality of conscious phenomena. The most important problem in philosophy, then, is that of the nature of the 'matrix' of our conscious phenomena. "How does conscious experience, gathered piecemeal and erratically in the course of life, become so integrated in latency as to form a more or less systematized potential totality, the integrant constituents of which are ever ready, when occasion occurs, to emerge duly ordered into present awareness?" The conscious states, being utterly evanescent, cannot give the permanent substratum for the integration, which must take place, therefore, outside consciousness; and the question is whether, having no other data than those of the moment of conscious awareness, we can infer therefrom the existence and nature of what underlies the integration of conscious experience.

Now, it is clear that our 'moment of conscious awareness' cannot affect other beings. We do not perceive one another's conscious states; and if we consisted only of what consciously appears, we should be imperceptible to one another. And if things consisted in the awareness of a supreme intelligence, they would also be imperceptible. These considerations give proof of the existence of a perceptibly revealed sphere of extra conscious existents, and the characteristics of our percepts symbolically represent these. Now, the author asks whether these characteristics of our bodily organism do not give us information regarding the integration of experience. There is the strongest evidence, the author claims, that conscious states are the outcome of brain activity. He cites in proof of this the dependence of special conscious states on special portions of the brain; also the fact that physiological psychology presupposes that 'sense-stimulating' and 'sense-stimulated' agents exist; that the former affect the latter, arousing percepts, and that modifications of the stimuli are followed by modifications of brain activity and then of consciousness. The author reaches the conclusion that "the specific activities of extra-conscious existents stimulate in definite ways the organic sensibility of our own entire conscious being. This specifically attained action and reaction it is which gives rise to the corresponding conscious states." "Consciousness, emerging thus solely under the functional activity of what is perceptually realized as brain structure, seems to be, therefore, exclusively an outcome of it." In the remainder of the article, which lack of space forbids us to outline, the author seeks to show how, by the same organic process in a progressive development, nerve structure has become integrated, accompanied by a corresponding integration of consciousness. C. W. HODGE.

SOCIAL PSYCHOLOGY.

(1) *L'Année sociologique, 1894.* M. LAPIE. *Revue de Metaphysique et de Morale.* May, 1895.

(2) *La Logique sociale.* G. TARDE. Paris, Alcan, 1895. Pp. XIV + 464.

Lapie claims that the general characteristic of sociological discussion during the past year is an increasing tendency to emphasize the psychological factors in history and the psychological, as contrasted with the biological, point of view for studying and explaining social facts. This finds illustration (a) in the growing dissatisfaction with the definition of society as an organism. Tarde urges that the

proper analogue is not an organism in general but a particular organ, the brain. Pioger (*La vie sociale, la morale, et le progrès*) would give the specific difference by calling it a *social* organism. Novicow (*Les gaspillages des sociétés modernes*) maintains that societies are rather to be interpreted as *êtres spirituels*. They are groups of ideas and sentiments. (b) The psychological tendency is seen in various definitions of what constitutes a sociological fact. Durkheim (see this REVIEW for May, 1895,) specifies a certain class of psychical facts, viz., those due to constraint. Lacombe (*De l'histoire considerée comme science*) regards as social those events which are either causes or effects of institutions. Lapie would correct this by pointing out that any idea becomes a social fact as soon as it is transmitted and so finds an echo in the consciousness of others. Tarde considers desires, and beliefs, if imitated, to be the central theme for social study. (c) The causes for social facts are to be sought in psychology. Le Bon (*Les lois psychologiques de l'évolution des peuples*) holds that the *character* creates the destiny of a people. Novicow and Lacombe maintain explicitly that social facts are explained psychologically and not otherwise—'wants act in history,' says the latter, 'not as biologically real but as felt solicitations' (Cf. Ward's *The Psychic Factor*). Durkheim is the most important objector to the principle of seeking psychological explanation, urging that the cause of a social fact can be only a social fact. I think, however, that the apparent opposition is due to the differing conceptions of the explanation sought. We explain by pointing out the preceding fact, or by analyzing the given fact into its elements (psychological processes). Each has its place.

M. Tarde's work supplements his preceding studies (*Lois de l'imitation*, etc.), and, as is indicated by its title, is a striking example of the present movement in French sociology traced by Lapie. Imitation is not the sole social fact. It is only the social memory, and memory while the foundation is not the edifice. The object of the book is 'to show the judgment and will at work in society,' to study the variations and inter-relations of beliefs and desires. As Kant's logic asked, 'How is knowledge possible?' so social logic asks, 'How is society possible?' and as Kant discovered certain categories necessary for the individual, so there are essentials for the possibility of society,—'permanent, necessary conditions of its more or less stable equilibrium.' These are either (a) logical, viz., language and the deity, or (b) teleological, good and evil. As space-time and matter-force are concepts for harmonizing sensations for the individual, so language is a medium for harmonizing perceptions, and the

deity for harmonizing thoughts and wills in society. Society began when the judgments and wills of individuals came consciously into contact, in agreement or discord, and there resulted a co-ordination of ideas and tendencies of the primitive family,—religion and domestic government. To understand the social processes we need then first a study of the laws which govern the conflict or coalescence of beliefs of varying degrees of strength. This yields quite a different classification of judgments from that of the traditional logic and a correspondingly different syllogism; and the conditions here analyzed are those most frequent in daily life and even scientific induction, for here the process is not one of drawing an inference from premises of equal certainty, but of a cumulative series of proofs resulting in increasing probability and stronger belief.

After a very suggestive treatment of this new logic of beliefs in his first chapter the author goes on to elaborate his doctrine of the social categories and to present analogies, some fruitful, some rather far-fetched (*e. g.*, when glory as social phenomenon is compared to self-consciousness) between the social and the individualistic phases of consciousness. The application of the principles developed to language, religion, the sentiments of the heart, political economy and art occupies the second half of the work. The role of imitation in all these departments is emphasized, but perhaps as a result in part of the criticisms on his earlier work, it is not over-worked, and the psychologist may find much valuable material in these chapters.

Univ. of Chicago. J. H. Tufts.

NEUROLOGY.

Die Localisationstheorie Angewandt auf psychologische Probleme. Beispiel: Warum sind wir "zerstreut"? G. Hirth. Einleitung von Ludwig Edinger. Zweite vermehrte Auflage. München, Hirth, 1895. Pp. XXIV + 112.

The extraordinary progress of minute histology of the nervous system within the last decade could not help arousing the interest of many speculative minds. While numerous attempts are made to fill the wide gap between the scanty data of localisation of motor and sensory areas of the cerebral cortex, and a possible localisation of the correlates of mental activity, it must become exceedingly difficult for one not following the strictly anatomical literature, to find out where the well established facts are at an end and where speculation looses touch with actual observation. Many papers are indeed so confusing that the present little work can be considered a very

fair exception, inasmuch as it does not often overstep the standpoint of speculation on a fairly sober basis. A few sanguine statements with regard to the correlation of anatomical structure and psychical function could easily be enumerated, but on the whole there is such a sound spirit pervading the little book that an occasional lack of conservativism should not prejudice even the pedantic reader. It may be that psychologists will not agree with many of Hirth's opinions; the remarks are in accordance with the monistic tendencies represented especially by the physicians of our days. As such they would indeed be worth much attention, and, if necessary, worth a thorough discussion by competent psychologists.

Hirth admits that, at present, it is impossible to construct even in outlines the process of thinking from our anatomical and physiological findings; but it seems certain that the problem of localisation and the development of the various systems of the nervous apparatus (Flechsig) will be a fertile field of study applicable also to the development of psychical systems.

In its development, every 'system' (Merksystem) has what might be called its special 'temperament', peculiarities in nutrition, tonus, energy and fatigue, owing to hereditary and developmental influences. This holds both for the well established systems in the specific areas of the cerebral cortex and for the material substratum of 'associative processes.' Flechsig's thought centers (a rather unfortunate term) may contain the most complicated ones of these systems; by all means we must maintain that, in some way, every mental process must have its localisation limited to some system of nerve-elements. The differences in the growth and ultimate architecture and functional energy of each system and the individual concatenation of the systems determines the individuality of the person. While lungs and kidneys and the heart are phylogenetically very old organs, their formation is less subject to variations; the older the systems are that form the basis of psychical correlates, the less they are subject to malformation, while the youngest acquisitions are most subtle and most perishable (the 'Achillesferse' of the human mind). The growth of these systems cannot be conceived without an hereditary disposition in the growth of the corresponding nerve-elements. The hereditary progress of the biological delusion (perhaps better the delusion of living beings) is next treated in a very attractive way; the outward projection, etc., the fact that normal brain activity is not felt as such any more than the activity of other organs, which too, we know by its results only; further the formation of pathological delusions, the necessity of the constant

correction of the 'Ichsynthese' by the stimuli from the periphery, the negation of a special apparatus or function of consciousness, the independent thought of special sensoria, unconscious parallel processes, polyideism, etc. The position taken in this sketch is next tested by, or applied to, the phenomenon of 'Zerstreutheit' (lack of concentration). It would not be fair to attempt a complete reproduction of the little pamphlet of 112 pages of text and 24 pages of introduction including Edinger's review of the first edition; it would be impossible to do it justice. The temptation is greater to give an outline of the actually available data of neurology which we could recognize as the present status with all its conflicts and problems. But for this, the limits of a review are too narrow.

The little book of Hirth may be heartily recommended as a very suggestive program of interesting psychological problems.

Sur les connexions du ruban de Reil avec la corticalité cérébrale. M. et Mme J. DEJERINE. Extrait des comptes rendus des séances de la Société de Biologie. Séance du 6 Avril, 1895.

During the last three years the question of the anatomy of the fillet has been the subject of an animated discussion. The course of the fibres originating in the nuclei of Goll and Burdach forming the central sensory path has become known through Edinger and Flechsig (1885). Before this, the view of Meynert was generally adopted; the sensory fibres, coming from the nuclei mentioned, would decussate and, joining the pyramidal tract, form the external bundle of the crus cerebri and from there enter the posterior limb of the internal capsule (carrefour sensitif of Charcot). Flechsig and Edinger showed that the fibres decussate, but instead of joining the pyramids, form the interolivary stratum and can be followed into what is known as mesial fillet. Forel (1877) followed the mesial fillet partly into the anterior corpus quadrigeminum (obere Schleife), partly into the optic thalamus (Thalamus-Schleife). Flechsig and his followers maintained (1881) that a part could be followed into the cortex of the parietal lobe, whereas Monakow (1884) furnished facts in favor of the view that the connection was not direct, but by means of the optic thalamus. For a rather full and excellently illustrated account of the anatomy of the fillet including the literature up to 1892 the reader may be referred to the Illustrations of the Mid- and Hind-Brain by Alexander Bruce.

The discussion has, of late, been led by Flechsig and Hösel on the one side, who, on ground of recent pathological observation, claimed a direct connection of the fillet with the cerebral cortex, and

by von Monakow and Mahaim on the other side, who bring material in favor of an indirect connection by means of the optic thalamus.

M. and Mme Dejerine have no less than 9 cases in which the fillet was involved and moreover 19 cases in which there were extensive lesions of the central and parietal convolutions, all examined by the method of serial sections. The following results may be gathered from their communications:

1. Lesion of the nuclei of Goll and Burdach (2 cases) are followed by (ascending) degeneration of the nerve-processes of their cells, *i. e.*, of the fillet. The degeneration cannot be followed beyond the subthalamic region and the inferior part of the optic thalamus.

2. Lesion of the fillet in the region of the pons (3 cases) is followed by ascending degeneration (which can be followed only as far as the anterior corpora quadrigemina and the inferior part of the optic thalamus, leaving completely intact the fibres passing through the lenticular nucleus, the nucleus of Luys, the globus pallidus, and the commissure of Meynert), and by descending degeneration (involving the interolivary stratum of the same side and the arcuate fibres and nuclei of Goll and Burdach of the other side.)

3. Lesion of the fillet in the region of the thalamus (4 cases) is followed by a small atrophy of the mesial fillet, diminishing downwards, as we approach the nuclei of Goll and Burdach and identical with the retrograde atrophy described by Forel (atrophe cellulipète). There is no reason to believe that the cells of the fibres that atrophy apparently downward must be located in the thalami; at any rate most of the fibres of the fillet come from the cells of the nuclei of Goll and Burdach.

4. Lesion of the motor and parietal area (19 cases, some of them with atrophy of the Linsenschleife), even when of old standing, do not affect in the least the mesial fillet.

This is a very complete refutation of the direct cortical termination as maintained by Flechsig and Hösel and their followers, and a complete corroboration of the views of Von Monakow and Mahaim.

Sur le méchanisme de l'agraphie dans l'aphasie motrice corticale. Ch. Mirallié. Compt. rend. d. de la Soc. de Biol. 30 Mars, 1895.

In motor cortical aphasia the disorders of spontaneous writing and of writing to dictation are proportional to those of the language spoken, while copying is not interfered with. For the explanation of this agraphia, Exner, Charcot and Pitres claim a special writing center in the foot of the left second frontal gyrus; Wernicke, Gowers, Lichtheim, Dejerine, Oppenheim and others say, however, that such

a center does not exist. If Charcot's view be correct, the patient is able to compound the words with letters written on cards, an action that does not involve any movements of writing. If, however, the agraphia exists because the idea of the word is altered in the 'language intérieur,' the patient cannot arouse the corresponding optic image (Dejerine) and he cannot put together the letters to form the word. The application of this test to 10 cases led to the following conclusions :

1. In cortical motor aphasia, the agraphia does not consist in the impossibility of tracing the letters on the paper and of grouping them in words. It comes from the impossibility of reviving in the mental language the conception of the letter and words, *i. e.*, from an alteration of the conception of the word itself. For this reason, a patient with agraphia cannot write better with the letters already written than when he has a pen in his hand.

2. Agraphia is therefore not the result of a motor disorder, of a loss of graphic memories ; hence it is not due to an alteration of a special motor graphic center specialized for the movements of the hand.

These views can easily be proven with a type-writing machine. The patient can write just as little with the machine as with the pen, although the type-writer does not require special writing movements.

The daily life of a protozoan: A study in comparative psycho-physiology. C. F. Hodge and H. A. Aikins. Am. J. of Psychology, Vol. VI, No. 4.

With a very ingenious method of registration the authors recorded the events in the life of various protozoans, especially Vorticella. They made the observations with the microscope, the animal being kept in a current of water from an aquarium containing ample food. The fourteen experiments lasted from a few hours to five and a quarter days, and the records are kept of the occurence of stalk contractions, reproductive phases and variations of the frequency of vesicle contractions, and of temperature and barometric pressure. The result was that as long as food was abundant, the Vorticella would move the cilia and reproduce by division without the intervention of a resting stage. Light or sounds of any kind as well as sudden changes of temperature (iced water), when not accompanied by a perceptible jar of the microscope, would not elicit reactions of any kind, whereas the sensibility to touch was manifested by very prompt reactions as selection of food and contraction of the stalk at the touch of an enemy or following a sudden jar.

The universe must consist for a Vorticella of a series of touches, possibly also of tastes and smells. The absence of fatigue seems to show that protoplasm may be formed as fast as used under favorable conditions of nutrition, and that with equally good facilities for the removal of decomposition products, these may not accumulate in amounts sufficient to interfere with activity.

Sur les connexions du noyau rouge avec la corticalité cérébrale. M. et Mme J. DEJERINE. Soc. de Biol., séance du 30 Mars. 1895.

Recherches sur la structure anatomique du noyau rouge et ses connexions avec le pédoncule cérébelleux supérieur. ALBERT MAHAIM. Mem. de l'Acad. Roy. de Méd. de Belgique, Tome XIII, 6ème fasc.

Mahaim's investigation is based on the results of the experimental degeneration method in the brain of rabbits. He finds that, contrary to the opinion of Bechterew, Marchi, etc., the superior cerebellar peduncle has its origin in the red nucleus and ends in the dentate body and the hemisphere of the cerebellum. A small portion does not decussate in the raphe of the tegmentum.

In the red nucleus of the rodents there is a small group of little cells which has nothing to do with the opposite cerebellar peduncle (nucleus minimus, at the junction of the anterior and middle third). The non-decussating fibres of the superior peduncle are derived from the cells of the anterior third of the red nucleus, the decussating ones from the middle and posterior third. The fibres from the middle portion give off very strong collaterals into the tegmentum.

The findings of Dr. and Mrs. Dejerine in brains with old lesions go much further. The atrophy of one cerebellar hemisphere associated with atrophy of the opposite cerebral hemisphere is a familiar occurrence. The question whether there is a direct connection between the superior cerebellar peduncle and the cerebral cortex, and whether the red nucleus has any connection with the cerebral cortex, has not been decided by the cases described in the literature.

It is certain that the superior cerebellar peduncle degenerates together with the cells of the red nucleus of the opposite side, when the dentate body of the cerebellum is destroyed; if, however, only the cortex of one cerebellar hemisphere is destroyed (cases of Menzel, Arndt and Dejerine) and the dentate body is not involved, superior peduncle and red nucleus remain intact: hence, there is no direct connection between the cerebellar cortex and the red nucleus. As to the connection between red nucleus and cerebral cortex the following facts are available: Mendel and Witkowsky reported degeneration of the red nucleus after lesion of the optic thalamus. The

cases of Flechsig and Hösel, Mahaim, Monakow and one of Dejerine's, old cortical and sub-cortical defects, can hardly be used for the decision of the problem because the degeneration of the red nucleus might be an indirect degeneration (Mingazzini), as all these brains had been affected in early childhood. Now, Dejerine is able to describe a case of cortical softening of the whole external aspect of the hemisphere and of the orbital surface of the frontal lobe, without any implication of the basal ganglia. The lesion was of eleven years' standing in a man of fifty-three, so that indirect degenerations would be very improbable. Besides other very remarkable degenerations which make the case a perfectly unique one, D. found degeneration of the fibres that enter the red nucleus from above. Hence, the connection between cerebral cortex and cerebellar cortex is formed by three neurons at least:

1. Cells in the cerebral cortex (largely the parietal lobe) with fibres radiating into the red nucleus.

2. Cells of red nucleus with processes forming the (decussating) superior cerebellar peduncle which ends in the dentate body of the cerebellum.

3. Cells of the dentate body sending fibres into the cerebellar cortex.

Sur une forme spéciale d' hémianopsie fonctionnelle dans la neurasthénie et la névrose traumatique. DEJERINE et VIALET. Société de biologie, séance du 28 Juillet, 1894.

Dejerine and his pupil Vialet (who died quite recently in the beginning of a brilliant career) publish two cases of neurasthenia worth knowing on account of the existence of a right homonymous hemianopsia associated with varying constriction of the remaining left fields. Their conclusions are as follows:

1. In certain neuroses, such as neurasthenia and traumatic neuroses, a persistent functional hemianopsia may occur.

2. While this form of hemianopsia does not offer very marked characteristics, it is generally distinguished by the variability of the limits of the preserved visual field.

3. Its diagnostic and prognostic value is the same as that of the constriction of the visual field.

4. Its medico-legal importance is considerable, as it cannot be simulated.

It is well to remember in this connection the case of bilateral hemianopsia inferior described by Dr. A. Hoche.[1] A woman of

[1] *Doppelseitige Hemianopsia inferior und andere sensorisch-sensible Störungen bei einer functionellen Psychose*, von Dr. A. Hoche. Arch. f. Psychiat., Bd. XXIII.

twenty-seven was taken with melancholia soon after the birth of a child; she had almost complete analgesia, loss of taste, loss of smell with hallucinations of smell, double hemianopsia of both lower quadrants on both sides, with hallucinations in the upper quadrants and peculiar photosmata in the lower (blind) quadrants; within five weeks she improved and made a complete recovery. This case shows clearly that every central lesion of vision can be psychogenous and that the psychogenous or at least functional character is not limited to the constriction of the field of vision or to amblyopia hysterica.

HOSPITAL, ILLS. A. MEYER.

The Localization of Cutaneous and Muscular Sensations and Memories. C. L. DANA. (Reprinted from Jour. of Nerv. and Ment. Dis., Dec., 1894.) New York: The Alliance Press. Pp. 27.

Dr. Dana collates evidence in favor of attributing sensory functions to the central cerebral convolutions. After citing some experimental data, he gives in detail the record of twenty-five clinical cases bearing on the question. In each of these the motor symptoms were accompanied by some sensory disturbance, the centres affected being located in the central convolutions. While many of these cases do not fulfil the most rigid requirements, still they furnish a certain amount of positive evidence to supplement the others. The writer does not attempt a discussion of the opposing evidence, but remarks that the number of "cases in which cortical injury occurs without apparent sensory disturbance . . . become yearly less." Admitting 'the sensory functions of the so-called motor cortex,' the question arises as to the nature of that function. Dr. Dana points out that in the great majority of cases the sensory functions lost are *localization* and '*active touch*'; these are really associative processes, depending respectively on tactile, and motor and tactile memories; they are "each associated with the special cortical motor centre for the particular part" involved. He concludes, therefore, that "muscular memories or association processes are represented, measurably at least, in the motor area." Hence the motor cortex is to be regarded as a 'sensory-memory-motor' organ.

Nachprüfung des der Theorie vom buchstabirenden Lesen und Schreiben zu Grunde liegenden Falles von Sprachstörung. R. SOMMER. Centralb. f. Nervenh. u. Psychi., 1894, N. F., IV, 113–137.

Dr. Sommer gives the record of some observations made by himself on a peculiar case of traumatic aphasia already reported upon by Prof. Grashey, with conclusions which differ from those deduced by the latter. The subject, Voit, fell from a ladder in 1885, strik-

ing on the left side of his head. He gradually recovered his physical health, and at the time of Sommer's examination had resumed his duties as worker in a brewery. The acute amnesia present at the time of Grashey's experiments had almost disappeared, but the aphasia remained. He was unable to name any object shown him without first writing its name. It was the act of writing, rather than the sight of the written word, that gave rise to the act of perception, as shown by the following: (1) Voit could name the object after going through the motions of writing, without marking, or when the hand that traced was concealed from his view; if his hands were held he would trace the letters with his feet, or even with his tongue, and succeed in getting the name; but if all these members were held fast he was unable to 'find' the word. (2) If the initial letter of the name was shown him among others, he was unable to identify it as such. The direct path between the optic and acoustic centres had apparently been interrupted, and the connection was supplied by a path through the visual motor centre.

Voit's mathematical powers were limited to multiplication of one-place numbers, which he performed rapidly, and simple addition and subtraction, which required some time.

The experiment was made of placing before him two objects belonging to a common genus; even when prevented from writing, and unable to 'find' their names, he signified (by nodding) that he recognized a connection or common ground between them. These results seem to show that "the *centre of ideation* (Begriffscentrum) is only the collective name for a number of different processes which may take place in quite different parts of the brain."

The case is important as a proof of the existence of a direct connection between the centre for the perception of objects (Centrum der Objectsvorstellungen) and that for the perception of graphic movements, which have been considered as connected only through the auditory centre. It also demonstrates, according to Dr. Sommer, that one part of an idea-complex may be lost without the connections between the rest being disturbed, and without the idea itself suffering any vital injury; thought may exist without language, in the case of both sense objects and ideas.

H. C. WARREN.

VISION.

La mesure des illusions visuelles chez les enfants. A. BINET. Revue philosophique, XX, 11–25. July, 1895.

The illusion of Müller–Lyer was the one chosen for measurement. One of its figures (B, the apparently shorter when both are really of equal length) was drawn in different lengths upon all but one of the right-hand pages of an album; upon the outer half of the last page, which could be unfolded wider than the others, was drawn the other figure (A, the apparently longer). A could then be compared successively with the different lengths of B, and one of the latter selected which should appear to be of the same length as A. Two such albums were constructed, showing the illusion on two different scales. Measurements were made upon 60 school children of the average age of 12, and upon 45 of the average age of 9 years. Results: (1) The illusion is stronger for the figures drawn on the smaller scale than for the larger ones. (2) The degree of the illusion depends on the order in which the lines are compared; it is greater when the series of figures B is followed in the order of decreasing rather than of increasing lengths. (3) The total illusion is the product of two illusions; namely, of the apparent lengthening of the line produced by the obliques at its ends in A and of the apparent shortening of the line produced by the obliques in B. These two effects are not of equal force, as can be shown by comparing each of the figures, A and B, with a series of straight lines. When A is 2 cm. long, B must be on the average 0.57 cm. longer, in order to appear of the same length. As compared with straight lines, A (2 cm.) shows an apparent lengthening of 0.51 cm.; B (2.57 cm.) shows an apparent shortening of only 0.13 cm. (4) The children have in general a vague consciousness of the illusion. (5) The illusion is stronger with children of 9 than with those of 12 years. For the former, with A 10 cm. long, there is an average apparent difference of 2.55 cm.; with A 2 cm. long, of 0.75 cm. For the latter, with A 10 cm., the apparent difference is 1.88 cm.; with A 2 cm., 0.57. The individual variation from these averages is considerable, reaching (for example) an average of 1.05 cm. for the younger pupils, with the larger A, and the series B taken in increasing order; which shows that the illusion varies greatly in strength for different individuals.

BROWN UNIVERSITY. E. B. DELABARRE.

Congenital Night-Blindness and Pigmentary Degeneration. COLMAN W. CUTLER. Archives of Ophthalmology, XXIV, 313–333.

König's identification of the distribution of brightness in the spectrum of the totally color-blind, and of the normal eye in a faint

light, with the absorption of the rod-pigment, which led to the self-evident consequence that the totally color-blind are rod-seers, as v. Kries puts it (*Ztsch. f. Psych.*, IX, 115)—a conclusion at which I had already arrived from theoretical considerations—renders any additional information as to night-blindness of peculiar interest. While the totally color-blind see with nothing but the rods, the night-blind evidently must have defective rods,—rods at least in which the rod-pigment fails to appear in accordance with the requirement of night-vision. This would doubtless be owing to a defective structure in the pigment-epithelium; and, in fact, Cutler finds it most probable, from anatomical considerations, that the defect, when congenital, is not of inflammatory origin, but is owing to some malformation in the pigment epithelium. This distinctly adds confirmation to the recent views on this subject. The defect has also this connection with color-blindness—that it is inherited, and that it is far more common in men than in women. It is of rare occurrence, only 54 well-described cases are recorded; of these, only two are isolated, the rest occurring in only ten different families; and there are 36 cases of males to only 18 of females. In two families five different generations have been afflicted in this way; in one the transmission has twice carried over two generations, the females being exempt; and in one, eight out of thirteen males all had it, the females again being exempt. It is also a significant fact that, while the fields of vision for white, red and green remain unchanged, that for blue, in the cases examined by Dr. Cutler, was in every instance contracted.

Ueber die percipirende Schicht der Netzhaut beim Menschen. W. Koster. Arch. f. Ophth., XLI (1), 1–27. 1895.

Koster finds himself unable to obtain a double shadow of a blood-vessel upon the retina by Prof. König's method, and Prof. Leber, of Heidelberg, as well. The double shadows they obtain, indeed, by H. Müller's first method for making the blood-vessels visible,—namely, if they concentrate light upon the sclerotic coat of the eye in two adjacent points by means of two lenses,—but they are not able to perceive that the distance between the two shadows varies when the light employed consists of the successive colors of the spectrum. Koster computes the ratio of the distance between two red shadows to that between two blue shadows, due to the difference in refrangibility of red and blue light, when König's method is made use of, and finds it to be not, indeed, a negligible quantity, 50:51, but less than the difference obtained by König by

observation, 22:19, and *in the opposite sense;* the different refrangibility of red and blue light would therefore have the effect of masking part of the influence of the distance apart of the different perceptive layers, if such exist, and hence could not be made use of to explain the phenomenon observed by König, even if it were sufficient in amount. [Koster, however, overlooks the fact pointed out in this Journal (II, 394), that the situation is changed according as the blood-vessel observed is between or not between the centres of the two overlapping images of the pupil, which are formed by the two holes in the moving diaphragm.] Koster awaits with impatience farther details in regard to König's experiment.

How Javal's Keratometer may be easily changed into a good Chromatometer for the Examination of Patients as to Color-Blindness. By CARL WEILAND. Arch. of Ophthalmology, XXIV, 3, 349–352.

Testing for color-blindness by means of colored worsteds is a proceeding of a very primitive character, and a spectroscope cannot readily be employed in the ordinary physician's office. The best instrument in which polarized light, a quartz plate, and a Nicol's prism are employed to this end is Chibret's chromatophotoptometer of 1885, but it is very expensive; and it is open to the objection that the color fields presented to the patient are very small and must be looked at with about 4D of accommodation. Dr. Weiland has devised a simple contrivance, consisting of a short tube holding a quartz plate and a Nicol's prism, which, if one has already a Javal's keratometer, would seem to accomplish everything that can be asked for; with the addition of a second Nicol, the brightness of the two complementary color fields can be varied, and a position of the instrument can be found in which they are, for the partially color-blind person, absolutely indistinguishable both in brightness and in color-tone. The instrument is first set for blue and yellow, which colors are both readily seen by the ordinary cases of color-blindness, and it is then rotated until the definite green or blue-green is found which the color-blind, of one or the other species, fails to distinguish from its complementary color,—seeing them both, in fact, whatever he may say if untrained, as grey. By a simple device dissimulation is rendered impossible. This most useful attachment can be furnished by Queen & Co., of Philadelphia.

BALTIMORE. C. LADD FRANKLIN.

SKIN-SENSATION.

Ueber die Trugwahrnehmung zweier Punkte bei der Berührung eines Punktes der Haut. V. Henri and Guy Tawney. Philosophische Studien, XI, 3.

The observation has been made that touching the skin with one point often gives rise to the perception of two. There are two solutions—that of Wundt and Müller, which is physiological; that of Camerer, Fechner and Nichols, psychological.

The subjects, Tawney and Stratton, were to observe and describe carefully the whole experience, besides answering 'one point,' 'two points' in the ordinary way. Two series were followed. In one the subject was ignorant of the nature and number of the stimuli; in the other he was informed. This gave the influence of knowledge and ignorance. The time between each experiment was not less than two minutes. There were two modes of experiment—pure double point illusions, where one point touched the skin; mixed, where two points touched. The volor side of the arm, midway between the wrist and elbow, gave the best results.

Results.—Sets 1 and 2. Pure illusions. Two points alternately touched with one point. The majority of tests showed the illusion. There were clear differences in the direction of the points and in their quality. Also the illusions stood in certain definite relations to the points touched. Sets 3 and 4: Pure and mixed. The relative number of illusions greater in the pure than in the mixed series. Illusion as to distance not beyond 20 mm. Qualitatively like points were more frequently felt than different. Sets 5 and 6: One point touched. Sometimes one, sometimes two points shown. Expectation influenced greatly the number and kind of illusions. The farther apart the points were expected to be, the farther apart they were felt. When two points were shown, the tendency was to feel two.

Conclusion: (1) The illusion is primarily bound up with the physiological processes. (2) The occurrence of the illusion is influenced in an important way by knowledge and expectation. This, however, does not explain all, *e. g.*, the constant differences in direction (transversely or longitudinally) and quality. Also in certain cases the subject expected and saw one point only but felt two.

Univ. of Chicago. S. F. McLennan.

Ueber Beziehungen zwischen geistiger Ermüdung und Empfindungsvermögen der Haut. H. Griesbach. Archiv. für Hygiene, Band 24, Heft 2, 1895.

Prof. Griesbach here reports a series of experiments in which he employed Weber's well-known method of measuring the fineness of

tactual discrimination with compass-points, as a means of determining the degree of mental fatigue. His subjects were, for the most part, scholars in the *Ober-Realschule* and *Gymnasium* in Mülhausen. A few supplementary series were also made on teachers, apprentices, factory-laborers and mechanics. Six areas of the hand and face were selected and the thresholds determined after periods of mental labor and rest. The uniform result was a marked increase in the diameter of the sensation-circles after effort, and a corresponding decrease after relaxation, and much more after rest. The following series will illustrate: 7 a. m., threshold 7 mm.; 8 a. m., after class in history, 12.5; 9 a. m., after Greek, 17; 10 a. m., after Bible-study, 9; 11 a. m., after Latin, 14; 12 m., after French, 17; 2 p. m., after two hours rest, 10.5. In the case of the laborers, whose work required far less mental effort, the increase is very much smaller, as appears by comparing the following series: 7 a. m., 10; 12 m., 11; 1.30 p. m., 10; 6 p. m., 11.5. On the ground of these results the writer criticises severely the German school program as much too exacting on the scholars.

LEIPZIG. CHAS. H. JUDD.

HABIT AND ASSOCIATION.

Gewöhnung und Gewohnheit: eine psychologisch-pädagogische Monographie. ARISTOTELES P. KURTIDIS. I–D. Athen, Konstantinidis, 1893. Pp. 64.

After describing the phenomena of habit, the author discusses the process of its formation with special reference to volition and the problems of education. The formation of habit depends, in the main, on repetition of impression; the proposal to regard persistence of stimulus as a second coördinate factor is rejected. The possibility of any habituation at all rests on the tendency in the organism to spontaneous action and on its capacity to react on external impressions: which two, automatic actions and impressions from without, are the 'prime movers' in the process. Which predominates in a given case, depends on the strength and frequency of the external influences and on differences of temperament. Habits may be inherited. In the development of habit, the order may be from the unconscious to the conscious or the reverse. The usual account is given of the latter and the inference suggested that habitual criminals should be less severely punished than others. The force of habit has its limitations in hunger, sleep, fatigue, etc., and in other habits. Where habits conflict, one as a rule gives place gradually; sometimes,

however, an old habit is broken and a new one formed on the instant. It may be worth remarking that the passage in Aristotle appealed to in support of this last statement (*de mem.* 2, 451 b 14) is probably corrupt. What Aristotle probably said was that "sometimes movements which occur but once become more effectually consolidated into a habit than others which take place repeatedly. Hence," he continues, "some things which we see only once we remember better than other things which we see many times." Our author finds here a distinct anticipation of the Herbartian doctrine of the 'memory of feeling or of will.' This conception of a 'memory of the will' forms, in connection with the foregoing analysis, the principle of the 'pædagogical exposition,' which is for the most part thoroughly sensible, but contains nothing especially new or in this place noteworthy.

Die Zweckmässigkeit der psyschischen Vorgänge als Wirkung der Vorstellungshemmung. OTTO LINDENBERG. Berlin, Duncker. 1894. Pp. 64.

The problem is, How are adaptations, which Darwinism explains for the species by natural selection, developed, under circumstances in which natural selection plays no part, in the individual? Limiting the enquiry to the psychological aspects of the problem, the present work seeks to show that the striving for an end tends of necessity to develop the means for its realization. The first to definitely state and treat the question was Lipps. And in principle his solution, namely, that the transition to the means is primarily due to a checking of the mental current, is correct. But he makes several unverifiable assumptions, and especially that the transition is brought about through its association of the means with the end inhibited. The theory to be presented differs essentially from that of Lipps in explaining the transition from association of the means with a presentation in consciousness.

The theory rests on the following general presuppositions. The facts of mental life are presentations and feelings. Feelings (will, pleasure and pain) arise from relations among presentations. Presentations arise productively from external stimuli and reproductively from contiguous association. These are the sole positive factors. Negatively they are determined by association, involving changes in the distribution of psychic force in the rise and fall of presentations, by the 'Enge des Bewusstseins' and by the inhibition of particular presentations by opposing presentations ('Vorstellungshemmung'). This last has very positive consequences. If, namely, in a strong

association *a–b*, *b* is inhibited, it follows, first, that the force of *a* in consciousness is strengthened, and, secondly, that the force which it would otherwise expend in reproducing *b* it now expends on other less strongly associated presentations. But among the latter those presentations will be in a situation specially favorable for reproduction which are either directly or indirectly 'means' ('Mittelvorstellungen') for the reproduction of *b* (*m* in *a–m* and *m–b*, *q* in *a–q* and *q–a* or *q–m*, etc.). For not only will they be strengthened by the influence from *a*, but they retain for themselves the force which, but for the inhibition, they would use up in the process of reproducing *b*. It follows, therefore,—and this is the kernel of the theory—that the very inhibition of a presentation tends to increase the force, and that in proportion to its completeness, of the presentations which are the associative 'means' of the reproduction of the presentation inhibited. It is assumed, of course, that the requisite associations exist. If they do not, or if they are too weak, the means-presentation will be inoperative; and this sometimes happens. Moreover, as the production of all contrast-presentations must be explained by the same principle, special circumstances must favor the reproduction of the 'means' which lead to *b* rather than those which lead to other contrast-presentations. These circumstances, however, we have: the actual concomitants of *a* in consciousness are variable, whereas the associations *a–m*, etc., are constant.

The foregoing sufficiently explains the reproduction of *b* where *b* is a mere thought-presentation. The interesting case is where *b* is a sensation. Its usual antecedents are then motor-sensations produced by actual movement. How is the movement produced? The answer is: as in the preceding case, by the influence of inhibition. Here, namely, not only the end *b* but the 'means' *m* (motor sensations) is inhibited. But the inhibition of *m* strengthens, on the principle, its 'means' associates. But the best possible association exists between *m* and the unconscious psychic factors which cause the movement. These factors, therefore, will be strengthened. As they do not require and never attain sufficient strength to rise to consciousness, we may conclude that, if the reproductive activity really strives to produce motor-sensations. that striving must lead, under normal circumstances, to the production of those sensations. If in any case the process is repeated, the associations will be strengthened and finally result in a permanent psychical structure. Actions may then take place by association alone without inhibition. In considering the genesis of actions, however, we must distinguish three cases: (1) the negative influence of association—new associa-

tions and directions of reproductive activity negating the original striving; (2) the positive influence of association, tending to destroy the negative influence of customary association in cases where the end involves novelty of form; (3) the influence of inhibition, leading to the desire for a sensation which is 'means' to another sensation we desire. No influence on action must be ascribed to pleasure and pain. The phenomena of inhibition are not to be explained by the feelings but the feelings by them.

The theory thus outlined is ably worked out in the essay, and, *granting the general assumptions*, goes far to solve the problem. It should at least serve to call attention to a point in the psychology of habit and volition which has hitherto been too little appreciated.

SMITH COLLEGE. H. N. GARDINER.

ETHICAL.

Vorlesungen über soziale Ethik. GEORG VON GIZYCKI. Aus seinem Nachlass herausgegeben von Lily von Gizycki. Berlin, 1895.

These posthumous lectures upon the subject of social ethics have special interest at this time because of the very recent death of Gizycki. They are an elaboration and application of the fundamental principle which underlies his general ethical system. This is the Greatest Happiness principle. He seeks to combine the concept of Oughtness with that of the Greatest Happiness. While acknowledging the full import of the categorical imperative, he insists that the only imperative is that which urges one ever to seek the greatest happiness of the greatest number. In the light of this guiding principle he reviews the present relations of capitalist and laborer, discussing questions of overproduction, strikes, woman and child labor, congestion of wealth, eight-hour laws, etc. Throughout he appears as the vigorous, undaunted, hopeful champion of the laborer. He especially urges the sympathetic and active coöperation of the brawn and brain laborers, insisting that theirs should be common cause, for the time is fast nearing when theirs will be a common lot and destiny. It is interesting to note the influence of our great American reformers upon his thought, as seen in the numerous quotations from Abraham Lincoln, William Lloyd Garrison, and Wendell Phillips. His hopes and plans for the future of the laborer, and the equality of privilege for all men, may be criticised as utopian, and yet cannot fail to impress the reader with their deep insight, large sympathies, and a profound belief in the possibilities of human nature. His work must indeed have left a

permanent impress upon German life and thought, and even legislation, as well as contributing valuable material to the general body of philosophic thought.

Une Étude réaliste de l'acte et ses conséquences Morales. T. WEBER. Revue de Métaphysique et de Morale. Sept., 1894.

The writer insists that our acts have in them often an element of spontaneity. Some decisions seem to come as veritable inspirations, just as works of the genius are said to be inspired. Conduct, which is the result of habit, is wholly reasonable and subject to laws logical, physical and psychological, but our spontaneities are free. The act is the concrete expression of the individual nature and must be so accepted and so regarded in its concrete capacity, and not to be arbitrarily estimated according to general laws. The act, since it has happened, must have a satisfactory explanation to account for it; if this were not so, the act could never have been accomplished. And yet we are not relieved of responsibility because our acts are but the outer expression of an inner nature. We are responsible for what we are. And so we are to be law unto ourselves. Our acts bear their own sanction in their consequences.

This study neglects the consideration of any norm of conduct as affording a suitable criterion for the estimate of individual acts. The author feels that whatever is is right. Morality without an ideal may summarily dismiss many perplexing questions to the convenient sphere of metaphysics, but yet the procedure is not permanently satisfying. This defect is felt throughout in reading this article.

Hétéronomie et Autonomie. E. DE HARTMANN. Revue de Métaphysique et de Morale. Mai, 1894.

The author indicates three possible situations concerning moral questions. That of pure autonomy, which disregards all heteronomous commands; that of pure heteronomy, which gives implicit obedience to a foreign will in all cases of doubt; and, finally, where the two principles act simultaneously, partly in agreement and partly in conflict. There may be an illusion where one thinks he is obeying a given law, when in reality he is but following a tendency of his own volition; or, on the other hand, he may think he is acting solely by autonomous determination, when he is really following that heteronomy due to his education, or through the unconscious fear of heteronomous authority, which is the rule of his decisions. Heteronomy may be of various kinds. It may be immanent in the

social life, becoming in time crystallized into a heteronomy purely legal and exterior. But even in this the individual has a part in formulating law, for this is but the integration of all morally autonomous acts of the many individual wills. The heteronomy, moreover, of a present generation was the autonomy of ancestors in former generations, and is an autonomy to the present so far as there exists an organic oneness with the past, inasmuch as they in common participate in the absolute reason. There is also a transcendent heteronomy looking to the will of the Supreme Being. This is more akin to autonomy than to the immanent heteronomy formulated in judicial systems. For if the absolute being which determines the objective moral ends is none other than the ego itself in its veritable essence, then the ends thus ordained become individual in a peculiar sense. The origin of transcendent heteronomy, historically considered, may be traced to the expression, through the prophets of different ages, of the popular autonomy at each period of civilization. There is a danger lest the true character of transcendent heteronomy, being discovered, it shall lead to the disclaiming of all obligation. This danger is, however, offset by the laws in which immanent heteronomy is formulated, compelling obedience by appropriate sanction; and thus absolute disorganization of society is averted. There has been a moral progress, historically, from the eudemonism of the ancients, though the heteronomy of the middle ages, to the autonomy ushered in with Kant and Fichte. While autonomy differs from heteronomy only as regards form, it differs from eudemonism in both matter and form. The value of heteronomy is that of a propadeutic to the final stage of autonomy. The proper work of Church and State, therefore, is not to educate the people to childish obedience of heteronomous commands, but rather to prepare them by appropriate training for the exercise of a true autonomy.

Untersuchungen über die verschiedenen Moral-Systeme. K. Leimbach. Fulda, 1894.

The author examines the several moral systems in reference to the relative positions they assign to law and freedom, in terms of the various probabilities for and against one or the other. He, thus notes the following systems :—absolute tutiorismus, laxismus, relative tutiorismus, probabiliorismus, æquiprobabilismus, and simple probabilismus. The last he regards as the true expression of ethics, both theoretical and practical, and defines it by the following rule:—In all cases of doubt between law and the free activity of the individ-

ual, when there exists a real probability favoring his free course of action, it is to be followed rather than the formulated law. It is the law. This theory is supported by three considerations. First, it allows for the supremacy of conscience. Second, it places freedom before law, as it is essentially. Third, it distinguishes between validum and licitum; the criterion of the former is to be found in terms of the certain; the latter, in terms of the probable. From the author's standpoint, which is evidently ecclesiastical, he finds a guiding thread which he is satisfied will lead him through the maze of the subtle problems of casuistry. This discussion throughout has a scholastic flavor, and abounds in quotations from patristic philosophy and church councils, as final authorities.

PRINCETON. JOHN GRIER HIBBEN.

ABNORMAL.

Jeanne D'arc. Vom psychologischen und psychopathologischen Standpunkte aus. JOS. ZÜRCHER. Inn. Dissert. (Zürich). Leipzig, 1895.

With the support of Prof. Forel, of Zürich, the authoress endeavours in the above study to render intelligible, from a psychological and psychopathological point of view, the unique personality of the gifted child of nature from Dómrémy. Besides the records of the process of rehabilitation and of the contemporaneous chronicles, (in so far as the contents of both are virtually guaranteed) and the works of Vallet de Viriville, Beaurepaire, Eyrell and Mahrenholz, the sources consulted were principally the Latin and Old French records according to Ouicherat. From the last named the authoress has drawn a valuable extract which occupies 50 pages of her work and consists of the personal testimony of Joan with respect to her visions and voices. It is in the original tongue with appended German translation.

In chapter I. the authoress gives a sketch of the life of the young heroine, of her triumphal march and her cruel, tragic end. In Chapter II. an attempt is made to shed light on the personality of Joan of Arc, as yet so slightly comprehended from a psychological and psychiatrical point of view in accordance with experiences of the theory of suggestion; and to classify this personality psychiatrically. An eminently gifted girl of unusual intelligence, deep moral earnestness and intense devotion to the dogmas of her church, she is, at the same time, an habitual religious hallucinatrix and auto-suggestionist. Her auto-suggestions have root in an old national tradition originating with the Keltic wizard Merlin, according to which a pure virgin from an oak-forest on the borders of Lorraine should free her country

after it had been brought to the verge of destruction through a woman (according to popular belief, the unnatural mother of Charles VII. of France.) She believes herself to be the chosen of God, her angels and saints prompt her with increasing vehemence to fulfil her divine mission: thus arises before us the symptomatic picture of systematic monomania. According to the authoress Joan of Arc was not the victim of actual mental disease. What in her is pathological—for instance, the variety and number of her hallucinations more especially—belongs to the pathology of genius which borders so closely upon insanity. 'In the genius of Joan of Arc lies the key to her story." "She is a genius of the first rank, perhaps the greatest of known female geniuses in the history of the world.' Her great intellectual gifts, the logical sequence of her thoughts and the absence in her of ethical defects characteristic of chronic paranoia, distinguish her, according to the authoress, from this class. Very impetuous and filled with ecstatic ideas, the maid disregards the reality and imperfections of life; she must, therefore, like all incorrigible idealists, finally tread the path of martyrdom.

After the above we need scarcely remark that with respect to the theory of suggestion the authoress advocates the views of the school of Nancy as opposed to those held at the Salpêtrière.

Though the style of the work is occasionally rather diffuse and not free from repetition, the train of thought in the second part lacking more especially in sustained clearness and precision, the authoress' merit in having investigated this interesting subject for the first time by the light of modern scientific knowledge, in having freed the character of this gifted heroine from prejudice, and further, in having delivered a spotless saint from the church 'out of which there is no salvation,' must yet be fully acknowledged. Notwithstanding this, the question as to the pathological classification of Joan of Arc may not yet be considered as finally settled. Nor can I agree with the view which the authoress, following Forel's idea, takes of consciousness. In spite of all recent endeavours to prove in one and the same individual a double consciousness, a double personality, a double ego, we must yet emphatically insist that there is and can be only one undivided individual consciousness it may exhibit, however, a graduated series of states. Elsewhere I shall have an opportunity of treating this point more in detail.

LEIPZIG. FRIEDR. KIESOW.

The Pathology of Mind. H. MAUDSLEY. London and New York, Macmillan & Co., 1895. Pp. XI + 576. $5.

In this new edition of his well-known work, Dr. Maudsley practically presents a new book. The omissions are large and the addi-

tions many, while the topics generally are rewritten. The work is a clear, philosophical and in most respects admirable compendium of mental disease, and a good book for general psychologists to make themselves familiar with.

The most interesting part of the volume to us is Part I, which deals with 'The Nature and Causation of Insanity.' Dr. Maudsley recognizes two conditions of mental defect, which will serve to recommend his book to modern psychologists—*i. e.*, the hereditary condition of the patient and the social condition. Possibly too little weight is given to heredity as such, since very large emphasis is laid upon the transmission of the effects of personal life, such as sexual dissipation, alcoholism, etc. Yet the essential truth, that insanity means a bad hereditary strain, is presented at length. The social condition spoken of relates rather to the definition of insanity than to its cause; and it does seem that Dr. Maudsley has laid his finger on the true differentia of mental abnormality is making the sign of defect the patient's lack of complete harmony with his social surroundings. "Insanity means essentially, then, such a want of harmony between the individual and his social medium, by reason of some defect or fault of mind in him, as prevents him from living and working among his kind in the social organization."

The author also makes the point very clear that social conditions may be the controlling ones in putting in evidence on the one hand, or in effectually hiding on the other hand, an individual's inherent weakness. Social conditions often 'prop-up' and make socially continent a man of essentially bad heredity: and it is just as true that social strain may 'touch-off' a man of slight abnormal variation and make him a lunatic. An interesting section is that (pp. 78 ff) which lays emphasis upon the social aspects of crime.

With such excellencies as these to commend it—and the treatment of Parts II–IV on the details of the insanities, which constitute the body of the volume, added on this side of the account—it is a pity that the author has not kept abreast of current psychology better than he has. He holds to the old flat-footed associationism, with the simple reflex-theory of nervous action, to explain all psychoses by. And, as of old, so here also, Dr. Maudsley appeals to nervous action as the final term of explanation. A little reading of Pierre Janet, whose name does not occur in the index, would inform Dr. Maudsley of the way the more difficult mental abnormalities may be approached on the mental side. But even to appreciate such books as Janet's, Maudsley would have to know something more of the mental processes which go with attention, sentiment,

volition, and all the phenomena of segregation, abnormal syntheses, etc., than the old English associationism knew. For example, compare Dr. Maudsley's crude note on the 'Subliminal' (pp. 118–119) with Janet's patient treatment of the stigmata which illustrate it. Perhaps it is a practical aspect of this theoretical defect that leads Dr. Maudsley to show the pitiable *heartlessness* that he does in alluding to religion, and even to certain of the more refined bearings of morality. So great is this defect, taken as a whole, that the book must be looked upon as one of the latest and best works of a school of writers which is just about to be 'turned down' in the historical progress of the science of abnormal mind. For if we are learning anything in this sphere in these late years, it is that the adequate understanding of anomalies of *sentiment*—personal, social, ideal—is to supply the superstructure to the foundation long ago laid by the psychology of association. But the appreciation or elucidation of sentiment is not Dr. Maudsley's forte.

J. M. B.

NEW BOOKS.

The Growth of the Brain. H. H. Donaldson. London, Walter Scott; New York, Scribners, 1895. Pp. 374. $1.25.

Die Seele des Kindes. W. Preyer. Vierte Auflage, Leipzig, Grieber's Verlag, 1895. Pp. XI+462.

Psychology in Education. R. N. Roark. New York, American Book Co., 1895. Pp. 312. $1.

Introduction to Philosophy. Fr. Paulsen. Translated from the third German edition by F. Thilly, with preface by W. James. New York, Holt, 1895. Pp. XXIV+437. $3.50.

The Principles of Morals. T. Fowler and J. M. Wilson. Oxford, Clarendon Press; New York, Macmillans, 1895. Pp. XXI+138+370. $1.90.

A Textbook in Physiology. M. Foster. Revised and abridged (in one volume) from the author's work in five volumes. New York and London, 1895. Pp. XLVIII+1183. $5.

Die Seelenkunde der Menschen als reine Erfahrungswissenschaft. M. Benedikt. Leipzig, Reisland, 1895. Pp. 372.

Tempérament et Caractère selon les Individus, les Sexes, et les Races. A. Fouillée. Paris, Alcan, 1895. Pp. 378.

Hedonistic Theories from Aristippus to Spencer. John Watson. Glasgow, Maclehose; London and New York, Macmillan, 1895. Pp. XIII+248. $1.75.

Studies in the Evolutionary Psychology of Feeling. H. M. STANLEY. London, Sonnenschein; New York, Macmillans, 1895. Pp. VIII + 392. $2.25.

Selections from Plato for English Readers. From the translation of B. JOWETT. Edited by M. J. KNIGHT. Oxford, Clarendon Press; New York, Macmillans, 1895. 2 vols. Pp. XXXVII + 242, VII + 245. $3.50.

The Psychology of Number. J. A. MCLENNAN and J. DEWEY. New York, Appletons, 1895. Pp. XIV + 309. $1.50.

NOTES.

"With the September number, the *American Journal of Psychology* enters upon its seventh volume. The preceding volumes (1887–1895) have been edited by President G. Stanley Hall (Clark University). For the future, the editorial responsibility of the *Journal* will be shared by President Hall, Professor E. C. Sanford (Clark University) and Professor E. B. Titchener (Cornell University). A coöperative board has been formed, which includes the names of Professor F. Angell, Professor H. Beaunis, Professor J. Delboeuf, Dr. A. Kirschmann, Professor O. Külpe, Dr. A. Waller, F. R. S., and Professor H. K. Wolfe. The *Journal* will be devoted exclusively to the interests of experimental psychology (psychophysiology, psychophysics, physiological psychology, etc.). Each number will contain, as heretofore, original articles, reviews and abstracts of current psychological books and monographs, and notes upon topics of immediate psychological importance. Contributions may be addressed to either of the three editors."

We take pleasure in inserting the above notice at the request of Professor Titchener. In the meantime the Sept. No. of the *American Journal* has come to hand. Its title-page reads in part as follows: "edited by G. Stanley Hall, President and Professor of Psychology, Clark University, assisted by E. C. Sanford, Clark University, and E. B. Titchener, Cornell University. With the coöperation of etc., etc. . . . . Clark University, Worcester, Mass. J. H. Orpha, Publlsher." We are obliged, in justice to the table of contents of the first two volumes of THE PSYCHOLOGICAL REVIEW, to protest against the following sentence from the 'Editorial' of this number of the *Amer. Journal:*

"I. The results of experimental investigations in psycho-physic laboratories. To this *Archiv* function, not yet represented by any serial publication in this field in English, etc."

Professor A. RIEHL of Freiburg has accepted a call to the University of Kiel.

Dr. JOHN BIGHAM has been appointed Professor of Philosophy in De Pauw University, Greencastle, Indiana. Work will be done in experimental psychology.

Dr. W. G. SMITH (Edinburgh and Leipzig) has been appointed Associate Professor of Philosophy in Smith College. He will have charge of the work in experimental psychology, for which the trustees recently made an appropriation.

The *Revue Néo-Scolastique* has begun a general bibliography 'des Ouvrages et des Revues de Philosophie' under the title *Sommaire Idéologique*, beginning with the literature of 1895. The first instalment, giving 595 titles, appeared in the July issue.

All communications for the editors of the PSYCHOLOGICAL REVIEW, together with publications intended for review, should be sent during the year beginning November 1st, 1895, to Professor J. McKeen Cattell, Garrison-on-Hudson, New York. Authors are especially requested to make the sending of reprints, papers, &c., as prompt and general as possible.

The second number of *The Psychological Index*, being a bibliography of the literature of Psychology and cognate subjects for 1895, will be issued on February 1st, 1896. It is sent free to all subscribers to the PSYCHOLOGICAL REVIEW. By arrangement with this REVIEW, it will also be published in France as part of the *Année Psychologique*. When authors are not able to send their publications to the REVIEW, the receipt of the titles, with name of author and publisher, place of publication (or name of Review or Archives), and number of pages, will assure their proper insertion in the *Index*. Communications for the *Index* should be sent to Dr. Livingston Farrand, Columbia College, New York, or to Mr. Howard C. Warren, Princeton University, New Jersey.

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